

What Future for British Broadcasting ?

SIR JOHN EDEN, the Minister for Posts and Telecommunications, must announce soon whether or not the government will set up an inquiry into the future of broadcasting. Time is getting short. The charter of the British Broadcasting Corporation and the licence of the Independent Broadcasting Authority expire in mid-1976. The licences for the companies like Rediffusion and British Relay which sell cable or wired television service also run out at the same time and so does the contract between the BBC and the Open University. Already the IBA is in the awkward position of preparing to award franchises for commercial radio stations which could—depending on when the three-year radio licences actually begin—extend beyond the limits of the IBA's statutory life. The usual length of the television authorities' franchises has been 12 years. So whatever decisions are taken by Parliament about broadcasting in the next year or two could fix the shape of Britain's television until 1988.

The report the government was ostensibly waiting for (see *Nature*, 241, 1; 1973) was delivered to Parliament a year late, in December. It was slipped in after questions on the Wednesday before Christmas, and the pressure is on Sir John to say what will happen next. But it is a good guess that the delay means that there is to be no formal inquiry at all. The strongest rumour so far is that the government will submit legislation extending the franchises of the BBC and the ITA until 1981.

There are other possibilities. The government is having to take into its calculations the fact that there must be a general election before June 1975. If it were to extend the lives of the BBC and the ITA for one extra year, until mid-1977, then there would be time for a full committee of inquiry to be set up now and continuing working uninterrupted (provided the Conservatives won again) through the election tension, preparing the recommendations about the long-term future of broadcasting. Or the government might appoint a small committee, a few wise men, to study the subject without hearing the usual vast array of witnesses, and then go ahead with its legislative proposals.

It does seem unlikely that the government would undertake to set the course of broadcasting for a dozen or more years without giving an airing to the many groups who hold passionate and conflicting views about how television should be organized. Several groups have already decided to go ahead on their own. The Social Morality Council has a committee, under the chairmanship of Dame Margaret Miles, already at work. This committee, which meets in private and heard Mr Charles Curran, director general of the BBC, at its session this week, intends to tackle the broad kind of question that the Annan Committee might have done. It will consider the relaxing of the monopolies of the BBC and the ITA, for example, and will also compare British television with services abroad.

The 76 Group, founded several years ago by discontented television professionals within the BBC and

ITA, is about to announce the formation of its own independent inquiry into the future structure of British television. The group, now led by Mr Philip Whitehead, MP, and including Mr Stuart Hood and Mrs Doreen Stephens, formerly of the BBC, was instrumental in leading to the establishment of the Annan Committee.

The Action Society Trust will also wade into the subject and, convinced (as is the 76 Group and many critics), that the TAC's report was inadequate, may concentrate on the new technical developments in communication that Britain should now begin to consider. Yet another inquiry is just being completed by the General Synod of the Church of England. Its emphasis has been on possible changes in religious broadcasting.

Perhaps a more critical question than whether there will be an inquiry or not is how seriously Sir John Eden takes the TAC's report. He has visited the United States recently and should be aware of the contrast between the cable television there and the vagueness about it in the United Kingdom.

Roskill Disinterred

THE British Government appears to be having second thoughts about the decision, itself much delayed, to build a third international airport for London in the Thames estuary, on Maplin Sands. In response to protests from the Opposition and many of its own supporters in the House of Commons that the airport is a potential environmental nuisance, and unnecessary as well, the government has decided that the bill authorizing developments at Foulness should be sent to a Select Committee of the House of Commons—a procedure that will allow reasoned objections to be made by outside interests but which is almost certain to delay a final decision until much later in the year. In the circumstances, it is no surprise that local authorities such as the Liverpool City Council are asking that consideration should be given to the development of an existing provincial airport instead of the development proposed in the Thames estuary, and no doubt this would be a convenient way of shedding some of the burden on local rates. It is more surprising that Mr Anthony Crosland, the Opposition's spokesman on environmental matters, should have lent his support to a reconsideration of the Foulness decision, for it was his sleight of hand which originally diverted the government and the British Airports Authority from swallowing the recommendation of the Roskill commission that the third airport should be built at Stansted.

The decision about the third airport is not merely a parochial matter but a teasing issue in environmental management. There are two questions to be decided—when will a third airport be needed and where should it be put? The Roskill commission estimated, in 1971, that existing



airports around London would be saturated by 1981 and that, by 1983, the cost of diverting aircraft from crowded airports would exceed the financial benefits to be gained by delaying a decision on a third airport. Its own preference was for a third airport to be in service by 1980, if only because of the danger that traffic might grow more quickly than had been forecast. In the event, however, the past two years have not been quite as bullish as originally foreseen. In the circumstances, there is at least good reason why the House of Commons should now take another look at the growth of traffic into the London area, and it is just conceivable that a decision could fairly be postponed until 1974, at least if those who live near the existing airports are prepared to tolerate for another year the nuisance to which they are already subjected.

The location of the new airport is a more contentious issue. Few would deny that Foulness is the best of all the sites in the neighbourhood of London considered by the Roskill commission. It would be a nuisance to fewer people than any other new airport in the metropolitan area. Moreover, its cost is only marginally and even problematically greater than that of an inland site, while there are obvious possibilities of grafting on to an airport in the Thames estuary industrial and commercial facilities which would be thoroughly unwelcome elsewhere. So much seems clearly to be recognized by the local authorities directly concerned, who appear to welcome the prospect of a development at Foulness. But there are, of course, some important disadvantages of the site. Several miles of mud flats will be replaced by artificial reclamation. Some but not all of those who live nearby will be afflicted with uncovenanted nuisance and there is even a possibility that the development of a major airport in south-east Essex will permanently change the character of what has previously been in part a rural area. On balance, however, if the need for air transport and the prospect of continued growth are acknowledged, it seems inevitable that the development at Foulness should go ahead—and luckily, the sense of injustice from which local inhabitants may suffer should at least be made less acute by the Conservative government's more liberal legislation on compensation for those adversely affected by public works. In this sense, the issue boils down to a simple conflict between the needs of the community as a whole and the interests, legitimate though they may be, of those who live nearby and who happen not to want an airport there. The House of Commons will do its reputation further damage if it fails to inform the coming argument about a third London airport with this simple and familiar principle.

But is there any chance that the need for a third London airport could be made as if by magic to go away? The Roskill commission was prevented by its terms of reference from considering more radical alternatives to the development of a third airport—Shannon in the Republic of Ireland or one of the possible sites in France might, for example, be as suitable as international feeder terminals as Liverpool. It is entirely conceivable that such terminals would be a more efficient means of helping people to embark on international journeys, especially if steps were also taken to rationalize the timetables of the international airlines, which at present compete for peak hour traffic and in the process exaggerate the problems of existing airports at these same times. The trouble is that any such solution to the problem of how to carry passengers to

destinations in the United Kingdom would require not merely a reorganization of the relationships between governments and the commercial airlines but the development of inter-city air traffic within Britain on a scale not yet foreseen. Whether the environmental nuisance would thereby be reduced is anybody's guess, but the outlook is not cheerful. And certainly there is no prospect that these complicated matters could be tackled in time to meet the demand for air travel in the early 1980s.

100 Years Ago



Inherited Instinct

THE following letter seems to me so valuable, and the accuracy of the statements vouched for by so high an authority, that I have obtained permission from Dr. Huggins to send it for publication. No one who has attended to animals either in a state of nature or domestication will doubt that many special fears, tastes, &c., which must have been acquired at a remote period, are now strictly inherited. In the following case we see a specialised feeling of antipathy transmitted through three generations of dogs, as well as to some collateral members of the same family, and which must have been acquired within a very recent period. Unfortunately it is not known how the feeling first arose in the grandfather of Dr. Huggins's dog. We may suspect that it was due to some ill-treatment; but it may have originated without any assignable cause, as with certain animals in the Zoological Gardens, which, as I am assured by Mr. Bartlett, have taken a strong hatred to him and others without any provocation. As far as it can be ascertained, the great-grandfather of Dr. Huggins's dog did not evince the feeling of antipathy, described in the following letter.

CHARLES DARWIN

"I wish to communicate to you a curious case of an inherited mental peculiarity. I possess an English mastiff, by name Kepler, a son of the celebrated Turk out of Venus. I brought the dog, when six weeks old, from the stable in which he was born. The first time I took him out he started back in alarm at the first butcher's shop he had ever seen. I soon found he had a violent antipathy to butchers and butchers' shops. When six months old, a servant took him with her on an errand. At a short distance before coming to the house, she had to pass a butcher's shop; the dog threw himself down (being led with a string), and neither coaxing nor threats would make him pass the shop. The dog was too heavy to be carried; and as a crowd collected, the servant had to return with the dog more than a mile, and then go without him. This occurred about two years ago. The antipathy still continues, but the dog will pass nearer to a shop than he formerly would. About two months ago, in a little book on dogs published by Dean, I discovered that the same strange antipathy is shown by the father, Turk. I then wrote to Mr. Nichols, the former owner of Turk, to ask him for any information he might have on the point. He replied—'I can say that the same antipathy exists in King, the sire of Turk, in Turk, in Punch (son of Turk, out of Meg) and in Paris (son of Turk, out of Juno). Paris has the greatest antipathy, as he would hardly go into a street where a butcher's shop is, and would run away after passing it.

WILLIAM HUGGINS

From Nature, 7, 281, February 13, 1873.

OLD WORLD

Euratom Reprieved at Dawn

AT a fourteen-hour meeting that ended at 6 o'clock on Tuesday morning the nine science ministers of the European community finally agreed a four-year programme for Euratom.

The three-part agreement, which will cost a total of 160 million units of account (about £66 million), puts an end to the five years of uncertainty about Euratom's future.

The compromise agreement will save the jobs of 1,650 of the staff of the four joint research laboratories at Ispra in Italy, Karlsruhe in West Germany, Geel in Belgium and Petten in Holland, the last of which was under threat of closure.

The four-year formula provides that 140 million units of account will be spent on a common programme of advanced technology projects ranging from environmental work to applied data processing and reactor safety.

Although this programme is common, not all nations will participate in all parts of the project. Work on the Petten high flux reactor will be shared between Holland, Belgium and West Germany only, France will not be involved in work on the control of fissile materials and Italy will not contribute to research on plutonium at Karlsruhe. Otherwise the programmes in this part of the package are common. In all 1,440 staff will be employed on the first part of the settlement.

The second part of the package has an agreed budget of 20 million units of account and will provide work for a further 209 staff, but the precise research to be undertaken has not yet been agreed. About a dozen projects have been put forward by the commission under this heading, and the nine nations have to agree by May which of these they will support. Ispra's work on the use of hydrogen to generate energy is one of these optional projects.

The third part of the package is labelled "indirect action programmes" and deals with contributions that Euratom will make to various national research projects in the enlarged community. These also will be decided by the end of April and a number of Euratom staff will be seconded to national laboratories, a move that will help reduce redundancies.

The second part of the formula will be reviewed annually and the first part after two years, thus providing at least a measure of security for the 1,948 staff of Euratom, many of whom were threaten-

ing to stage a "work-in" on the lines of the Upper Clyde Shipbuilders unions if agreement on a realistic programme was not reached.

As it is, 98 of Euratom's scientists and administrative staff will be made redundant. Forty-eight of these will go because the programme has been cut from the five-year agreement that the commission wanted instituted; the remainder will make way for staff from Britain, Ireland and Denmark who only became part of Euratom in January.

The final programme is evidently something of a compromise between the commission's plans—which would have

produced a budget of 185 million units of account—and the views of Britain, Holland and France who were eager that Euratom should be pruned down.

Reactions from the ministers concerned were those of relief as they ended their lengthy meeting. The Belgian science minister, Mr Charles Hanin, said, "it has been a good night and we are at last emerging from a situation which has lasted many years". Mr Ralf Dahrendorf, the commission member who now has the unenviable task of actually putting the agreement into effect, was slightly more cautious. "This is a sort of turning point", he said.

NUCLEAR POWER

Vinter Report: Select Committee Acts

THE Select Committee on Science and Technology is to invite Mr Tom Boardman, Minister for Industry, to appear before it with a copy of the Vinter report to explain why the report cannot be published.

Officially known as the thermal reactor review and compiled under the chairmanship of Mr Peter Vinter, deputy secretary at the Department of Trade and Industry, the report is intended to help ministers decide which type of reactor should be built next.

The Select Committee, which has been examining the nuclear power industry for some months, has repeatedly asked that the report be published. Now it has attempted to use its right to send for "persons, papers and records", as the government has refused to part with a copy.

The committee—which last year published the Docksey report which the government had refused to make public—believes that there is an important principle at stake. It is quite willing to respect the commercial secrets in the Vinter report that the government has repeatedly cited as its reason for not publishing, but it feels that the government's refusal to divulge the thinking that lies behind the report is obstructing the committee in its work.

Mr Arthur Palmer, chairman of the subcommittee that is examining the nuclear power industry, made the point forcefully when the committee was taking evidence from the United Kingdom Atomic Energy Authority last week. On hearing from Mr F. J. Doggett, deputy chairman of the authority, that the authority knew of the Vinter report's contents, he

rejoined that he hoped people would note that a public corporation has information that has not yet been made available to Parliament.

Mr Doggett, questioned on the reorganization of the nuclear industry that the government has promised, said that one of the first tasks of the new company will be to establish links with a European company. The new company, he said, "should be left free to choose the best common partnership".

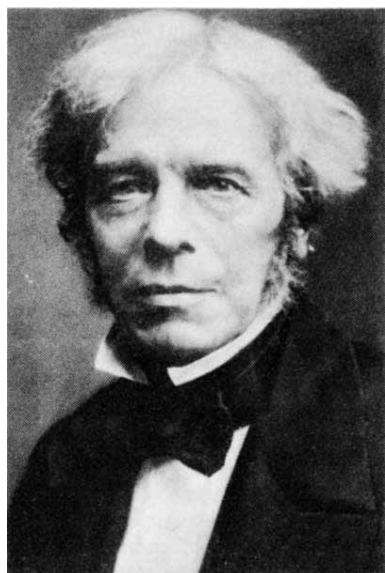
Mr Doggett's emphasis on European links reflected the views of Sir Edwin MacAlpine, chairman of the Nuclear Power Group, when he gave evidence to the select committee recently. Sir Edwin was also at pains to urge that the new company "should be free from any influence of manufacturers"—a remark that was taken to be a thinly-veiled reference to the government's apparent intention to install Sir Arnold Weinstock, managing director of GEC, at the helm of the new company. GEC supplies turbogenerators and other equipment for some nuclear contracts. Sir Edwin made it clear that he favoured a new company made up of three parts—his consortium, the Atomic Energy Authority, and the other consortium, British Nuclear Design and Construction, of which GEC is a part. The government, however, is believed to favour a company with strong central management rather than a merging of the consortia on the lines proposed by Sir Edwin. Sir Edwin, however, told the committee that as far as hopes for a European company go, it "would be a terrible disaster if any one company were seen to be in control".

HISTORY OF SCIENCE

Faradayia

MORE than 105 years after his death, the Royal Institution has finally paid tribute to Michael Faraday. Earlier this week the Queen, the Royal Institution's patron, opened a museum, an archive room and a refurbished laboratory, all bearing Faraday's name.

Professor Sir George Porter, the institution's director, repeated Faraday's chief experiments on electricity—including, with commendable energy, a hand-driven demonstration of electromagnetic induction with a light bulb, a coil and an iron bar—and the Queen unveiled a plaque on the floor below, initiating the curtain motor by an impulse from the original iron ring used by Faraday when he discovered electromagnetic induction. The ring worked, the curtain drew back and everyone applauded.



Michael Faraday

The Royal Institution has presented its treasures well. Probably the most complete collection of the work of one scientist, the Faraday museum contains models of the first electric motor, examples of early generators of Faraday's time, apparatus made by Newton for Faraday and such items as his "electric egg" used for the study of discharge in gases at different pressures. The archives room contains Faraday's diaries, all 16,000 entries, each one of which added something to scientific knowledge, and a copy of the probate of his will in which he wrote of his manuscripts "... these I offer for the library of the Royal Institution, if my managers should think them worth a place; if not they remain at the disposal of my executors".

The archives also contain manuscripts of Davy, Tyndall, the Braggs,

T. H. Huxley and Spottiswoode to name but a few. But the highlight of the displays is a reconstruction of the small laboratory in which Faraday made most of his discoveries. The room has been returned as far as possible to its nineteenth century appearance, with pieces of Faraday's apparatus dotted about the place, including the great horseshoe electromagnet he built. It is cleverly done, carefully lit, and looks as though Faraday has just left the room for a moment.

The Royal Institution may have been a long time making its Faraday collection available to the public, and it may be all of five years since the Faraday Centenary Appeal raised the money to carry out the transformation of the laboratory (in the form of a gift from the Camille and Henry Dreyfus Foundation), but it has been worth the wait.

HORTICULTURE

Mercury Substitute

A DEVELOPMENT which obviates the need to use compounds of mercury to dress seeds has been turned to commercial use by scientists at Rothwell Plant Health in Lincolnshire.

Traditionally, seeds have been dressed with mercury compounds before planting to avoid diseased crops. But even though the amounts of mercury compounds used worldwide for agricultural purposes are small—in 1968 slightly more than 2,140 tonnes were used, 1,600 tonnes in Japan, 400 tonnes in the United States and only 20 tonnes in Britain—there has been concern about the health of the people who carry out the dressing process.

During recent years there have been several attempts to develop a seed dressing as effective as mercury which is not toxic, and the dressings which have been developed were found to be effective for a particular variety of seed, but, generally speaking, no universal substitute for mercury has been marketed until recently.

In 1972, Rothwell Plant Health Limited of Lincoln marketed a dressing composed of a mixture of carboxin and thiram which, it is claimed, is as effective as mercury in preventing disease in cereal crops. But to buy a ton of seed dressed with the non-mercurial dressing costs £4 more than the mercury dressed seed. At present a farmer who wishes to buy a ton of wheat seed will probably pay about £60 or £64, depending on the dressing used.

Rothwell is also developing a hybrid wheat variety. Mr Giles Dixon, director of scientific development at Rothwell Plant Breeders, said recently that the aim is to produce an F_1 hybrid with a yield 25 per cent greater than non-

hybrid varieties at the time that the hybrid would be sold commercially. Mr Dixon also said that it would probably be five years before it was known whether the hybrid variety would be commercially viable.

SCIENCE MUSEUM

Props for the Provinces

A FUND of £150,000 has been set up by the government to help needy provincial museums to purchase and install scientific and technological material.

The fund is to be administered by the Science Museum and becomes available on April 2 this year. Its aim is to establish gradually a national pattern of acquisitions, avoiding unnecessary duplication and encouraging regional museums to build up specializations in subjects related to the particular industries and interests of their areas.

The terms of reference of the fund are broad. Any object of scientific or technological interest is covered—including those relating to the biological and geological sciences—and transport, restoration and installation all qualify for financial support.

Miss Margaret Weston, the Science Museum's new and young director, said this week that the fund, which is designed to avoid the loss of technological and scientific material that has taken place in recent years, "will help a lot", and she is looking forward to seeing what kind of material the provincial museums wish to save.

The fund is the result of a report presented nearly two years ago by the Standing Commission on Museums and Galleries which recommended that £200,000 a year should be made available to help the provincial museums. Although the Paymaster-General has only approved £150,000 for the first year, Miss Weston points out that the grant is renewable each year and may well be increased.

The granting of money to provincial museums is a matter in which science has lagged behind the arts. The Victoria and Albert Museum has had a similar scheme running for several years, and although it was quite willing to consider scientific and related objects, this is the first time a strictly scientific fund has become available.

When considering applications, Miss Weston said this week, the Science Museum will consult with the Geological Museum and the National Maritime Museum where necessary. Initial reaction among provincial museums is enthusiastic.

A hidden bonus for the Science Museum is that the new fund throws into greater relief the Science Museum's current acquisitions grant,

which is a paltry £8,000 a year. "That," says Miss Weston, "will have to go up." But not yet. The museum's budget is run on a quinquennial basis and there are still eighteen months to go before the new settlement is due.

UNIVERSITY RESEARCH

Physical Sciences Cut

THE growth of postgraduate numbers in the physical sciences is to be heavily restricted following the government's white paper on education published last November.

Now that the University Grants Committee has apportioned the £1,392 million (1972 prices) of recurrent grant that makes up the 1972-77 quinquennial settlement, certain of the white paper's provisions have become clearer, in particular the stipulation that the numbers of postgraduate students are to be cut from the current 19 per cent of full-time students to 17 per cent. This amounts to a 60 per cent reduction on the extra numbers of postgraduates requested by the vice-chancellors in their submissions to government.

Vice-chancellors are seriously concerned at this cut in the growth rate which will increase Britain's postgraduate numbers by only 7,000 to 52,000 by 1976-77, and they are also critical of the way post-experience courses, taught postgraduate courses and research degrees have all been lumped together in the calculations.

Guidance as to the numbers of postgraduates in each subject area that individual universities should aim at has been given to the universities by the UGC, and from these figures it is apparent that science and technology have suffered badly from the reduction in growth, physics and chemistry taking the largest cuts, with medicine, social sciences and business studies receiving the largest increases in postgraduate numbers.

A number of vice-chancellors are also seriously worried by government advice, issued through the UGC, that growth in the numbers of overseas postgraduate students taken by British universities should also be cut. Many of these come from the third world and it is felt that training overseas postgraduates in skills that can be used in their own countries is one of Britain's most helpful contributions to the developing nations.

The vice-chancellors also maintain that the universities must be given full compensation against inflation during the quinquennium. The settlement is a particularly tight one with the universities being asked to take more students at lower unit costs, and the vice-chancellors feel that this can only be done if inflation is not allowed to further cut the actual monies available.

SPACE

ELDO Lingers On

THE European Launcher Development Organization won a two-month reprieve at a council meeting in Paris last week. The council decided to keep the Europa II programme running until April 1 after France and Germany had again failed to agree over the long-term continuation of the programme.

Europa II is all that is left of the ELDO programme after the decision of the European Space Conference in December last year (see *Nature*, **240**, 516; 1972) to scrap Europa III in favour of a new French launcher proposal, L3S, which the French claim will be considerably cheaper than Europa III would have been.

The Europa II programme consists of two test firings during this summer and autumn, the second of which will carry a dummy load to represent the communications satellite *Symphonie I*.

Two firings of *Symphonie* are then planned, followed by a launch of an ESRO satellite to complete the Europa II programme. The French are eager to see the programme through, but the Germans are believed to favour scrap-

ping the launcher, which has proved a costly failure so far, preferring instead to send *Symphonie* up on an American Thor Delta launcher.

Last Friday's meeting did little to produce any solution to the disagreement, but the council did agree that the Europa II programme is to continue as planned until April 1. A final decision on the future of the programme, and therefore of ELDO, will be reached "in good time" before then.

ELDO's future as a separate organization is in any case limited. The European Space Conference held last December agreed to a British proposal that a European Space Agency be formed by 1974. Under this proposal ELDO is to be merged with ESRO under the control of the agency which will also oversee individual space programmes of the member countries. It is expected that this new agency will also be responsible for developing the L3S, a three-stage launcher which the French claim will have the same performance as Europa III but at a cost of Fr 2,200 million (about £180 million) compared with the estimated Fr 3,000 million that Europa III would have cost (see *Nature*, **240**, 434; 1972).

HOVERTRAINS

New Linear Motor

IT is now feasible both to levitate and to propel a hovertrain using only a linear induction motor. This was the crux of the Friday evening discourse delivered last week at the Royal Institution by Professor Eric Laithwaite, of Imperial College, London, one of the founding fathers of the linear motor.

Professor Laithwaite backed up his assertion by demonstrating the latest version of the transverse flux motor. This kind of motor, in which the flux is perpendicular to the direction of motion rather than along it, was evolved by Professor Laithwaite, Dr J. F. Eastham and the Imperial College group in 1968 and developed since then.

Although the new motor was first exhibited at a high speed transportation exhibition in the United States last year, the details were revealed for the first time last Friday. The unveiling is particularly timely because of the extreme uncertainty at present about the future of Tracked Hovercraft Limited, the wholly owned subsidiary of the National Research Development Corporation, which has now developed a hovertrain weighing ten tons, supported on an air cushion and propelled by a linear motor. This vehicle recently achieved a speed of 107 miles an hour on the company's 1-mile test track in Cambridgeshire; the average acceleration on this occasion was $\frac{1}{3}$ g.

The government has not yet decided whether or not to inject another £4 million into the hovertrain project, but Professor Laithwaite said this week that for this amount of money it would be quite possible, using the new motor, to build a 30-ton vehicle capable of carrying passengers and travelling at 250 miles an hour within three years. This would involve lengthening the test track to 3 miles. Professor Laithwaite also says that such a vehicle could hover at a height of 6 inches above its track. The present British hovertrain, the RTV 31, travels along with a clearance of only an inch or two.

Research on high speed transport of this kind is being actively pursued in several other countries, the United States, France, Germany and Japan for example. But whereas most people seem to be agreed that the linear motor, in some form, is an essential ingredient of any arrangement, there are differences of opinion about how vehicles should be made to hover. Germany favours a suspension system involving a feedback loop to maintain stability, whereas the United States, France and Japan are putting their money behind levitation based on superconducting magnets. Both these systems suffer from the disadvantage that they will probably prove much more expensive (in liquid helium for example) than a system that makes use of a transverse flux linear motor and thus requires no separate means of levitation or suspension.

CREATIONISM

Backing the Bible

ONE group in Britain that has been watching the activities of the California and Michigan State Education Boards' activities with more than average interest recently is the Evolution Protest Movement. The move by the two education boards to include the religious theory of creation (albeit in a very cursory form) in school textbooks has met with the cautious approval of the movement, which, since 1932, has been dedicated to the thankless task of getting British scientists and laymen to listen to its views.

The movement, which has 800 subscribing members, has three chief aims—to demonstrate that the theory of organic evolution is not in accordance with scientific fact, to show that evolutionary teaching causes a decline in true Christianity and to supply information supporting the Bible statements on creation as opposed to the widely accepted views of evolution. Their chief weapons are the book and the pamphlet, although members of the movement are always willing to lecture and debate anywhere.

Since 1932, when the movement was founded by Captain Ackworth, RN, a submarine commander who is reputed to have won a libel action against Churchill, more than 200 pamphlets and several books have been published by the movement, the lodestar of their work being Professor W. R. Thompson's introduction to the 1956 Everyman edition of *The Origin of the Species*. Certainly better argued and less discursive than most of the movement's work, Thompson's critique forms part of the scientific backbone of what is essentially a lay organization. Past presidents include Douglas Dewar, a one time auditor general of India, Sir Ambrose Fleming, the British physicist who first devised the electron tube, and Captain Ackworth, although the current president, Sir Cecil Wakeley, past president of the Royal College of Surgeons, is a man highly distinguished in a biological field.

Dr C. E. A. Turner, the movement's chairman, a retired chemistry schoolmaster with an interest in biochemistry and a doctorate in the history of scientific education, says that the movement's chief concern is to see "the other side" of the Darwinian coin put across. The movement imposes no set beliefs on its members, simply a generalized opposition to Darwinism, and Dr Turner is at pains to point out that he does not want to see the creationist theory imposed on schoolchildren. "We want to see that people are aware of the alternatives; the problem at present is that one side is pushed to the exclusion of the other." Dr Turner

also complains that "it is very hard to get anything into the press. If you write as a member of the movement people think you are cranky—classed with the flat-earthers or something—and as there are a number of competent scientists in the movement this is not fair".

EPM has links with the Creation Research Society in the United States, a body of research scientists in a number of disciplines who attack Darwinism on more strictly scientific grounds, although they are "committed to a full belief in the Biblical record of creation and early history".

EPM is a much more modest organization, producing its half yearly journal with pamphlet supplements, and holding its annual meeting each September. But membership has gradually increased and the present wave of controversy in California and Michigan, not to mention the correspondence columns of *Nature*, could give it the chance to make its views better known, although it has at present no plans to seize the day.

SOVIET SCIENCE

Luna-21 Lands

from our Soviet Correspondent

THE landing of Luna-21 and roll-out of Lunokhod-2 into the Mare Serenitatis area, at a site some 180 km almost due north of the Apollo-17 site, is already being hailed by the Soviet press as a precursor of cooperation in space between the United States and the USSR.

This international theme of Lunokhod-2 is also stressed by the presence of a directional laser reflector supplied by France which extends the joint Franco-Soviet research on measuring accurately the distance from Earth to Moon begun by Lunokhod-1 (*Nature*, 228, 795; 1970).

Although a full list of experiments has not been announced, the programme of Lunokhod-2 seems a repetition of that of its predecessor, including a spectrometer to determine the chemical composition of the surface of the Moon, a twin-channel directional photometer to analyse radiation from stars in the visible and near ultraviolet region (similar to those carried by Kosmos 51 and 213), a magnetometer experiment, instruments to test the mechanical properties of the surface of the Moon including a conical rotary penetrometer with cruciform blades and sensors attached to the chassis of the vehicle to measure the angle of inclination.

There are also panoramic television cameras with angles of vision $180 \times 30^\circ$ in the lateral and $360 \times 30^\circ$ in the vertical direction. The fact that Lunokhod-2 on its roving mission has covered more

than 1,100 m in the first 4 days, compared to Lunokhod-1 which moved a mere 125 m in the same time, indicates the emphasis placed on gathering as many photographs as possible from this transitional sea-to-highland area. And, once again, as in Lunokhod-1, the ambient conditions of the instrument chamber (temperature 20°C , pressure 80 cm Hg) suggesting a life support system, may be some indication that the instruments on board have been designed for use in manned missions.

IMPERIAL COLLEGE

Unemployed Scientists

IMPERIAL COLLEGE still retains its attraction for undergraduates in the sciences and engineering. In the college's annual report published recently, it is reported that 4,994 students applied for a total of 952 places at the college for the academic year 1971-72. But this figure is 12.1 per cent fewer than applied the previous year.

True to tradition, Imperial College still boasts one of the largest post-graduate bodies in Britain with 37 per cent of the students during 1971-72 involved in either research or doing advanced courses.

But in spite of the reputation of the college its graduates still found difficulty finding jobs. A survey shows that in December 1971, 5 per cent of that year's Imperial College graduates were unemployed and that the whereabouts of 7.3 per cent were unknown. In September 1972 the University Grants Committee published equivalent figures for the country as a whole (*Nature*, 240, 175) and it reported that 7.9 per cent of all 1971 graduates were unemployed in December 1971—with 6.7 per cent of applied science graduates still without a job.

In common with science graduates of other colleges and universities, Imperial College students had to diversify to find employment. The report says that some scientists and engineers now find employment in accountancy, banking, the actuarial profession and the Stock Exchange and that in 1971 13.1 per cent of the college's first degree students took up employment in legal, commercial and related areas compared with 3.1 per cent in 1965. Even 10.9 per cent of the college's higher degree graduates now find their way into these fields compared with 0.8 per cent in 1965.

But Imperial College has made efforts to improve the situation and during the past year the college's appointments board has set up contacts with companies in Europe and has increased contact with British companies by the simple expedient of arranging for members of the academic staff to hold consultancies in industry.

NEW WORLD

New Hope of Comprehensive Test Ban?

by our Washington Correspondent

THIS year marks the tenth anniversary of the signing of the treaty banning the testing of nuclear weapons in the atmosphere. It also marks the end of a decade of intensive underground testing and a decade of discussions and research concerned with extending the partial test ban to include all methods of testing nuclear weapons. But, while the governments of the United States and the Soviet Union have repeatedly reaffirmed their commitment to "seek to achieve the discontinuance of all test explosions of nuclear weapons for all time", a commitment enshrined in the preamble to the partial test ban treaty, they have not even come close to agreeing on a formula for implementing such a comprehensive agreement.

Early next week, however, a group of some two dozen senators, led by Kennedy, Mathias, Hart, Humphrey, Muskie and Case, will introduce a resolution into the Senate calling on President Nixon to set forth a new proposal for a permanent comprehensive test ban treaty. And, in the meantime, the resolution urges the President to propose a moratorium on weapons testing, to remain in effect as long as the Soviet Union also abstains from underground testing. The resolution calls for such steps because "early achievement of total nuclear test cessation would have many beneficial consequences; creating a more favourable international arms control climate; releasing resources for domestic needs . . . and complementing the ongoing strategic arms limitation talks".

Few would disagree with those sentiments, but the critical question is no longer whether or not a comprehensive test ban is desirable but whether it can be monitored and enforced with maximum assurance that it is not being breached, and without generating false alarms. That has always been the stumbling block in the path of negotiations towards such a treaty, and it will certainly become the central point in discussions on the Senate resolution.

The Administration is, however, now undertaking a fresh and important initiative to extend the theoretical basis for ensuring that a comprehensive test ban could be adequately monitored. The initiative involves improving the capabilities of some 20 seismic recording stations in the World Wide Standard Seismographic Network (WWSSN)—a network of seismographs around the world managed at present by the

National Oceanic and Atmospheric Administration—and the installation of up to three long-period seismic arrays in locations which have yet to be firmly decided. Such an upgrading of seismic recording capabilities was in fact proposed by the United States delegation to the Committee on Disarmament in Geneva last year, and planning is proceeding along the lines set out in the US working paper presented to the committee.

Upgrading of the WWSSN stations, which will be financed by the Defense Department's Advanced Research Projects Agency (ARPA), will consist of

equipping the stations with digital recording facilities, moving some to sites where they are less affected by background "noise" signals from the weather, traffic and other factors and in some cases by installing improved instrumentation. Another development will be more efficient transfer of data from the seismic stations to a storage and analysis centre in Virginia; data from some stations will be relayed by satellite to the United States. Planning is now in the final stages and the upgradings are expected to be carried out within the next few months.

The immediate objective is to pro-

MILITARY FITNESS

Sickle Cell Screening

by our Washington Correspondent

A COMMITTEE working under the auspices of the National Academy of Sciences has recommended that all potential recruits for the military services should be screened for sickle cell anaemia. Those who are found to have the disease should be excluded from service, but those who are found to carry only the sickle cell trait—the recessive gene mutation which generally poses little or no hazard to the carrier—should not be excluded. The committee's report, although it applies specifically to military policy, is an important statement in the growing controversy which surrounds screening policies for sickle cell anaemia.

The National Academy of Sciences was asked to draw up recommendations for the Department of Defense last year, and the chief conclusions to emerge from the resulting study are, first, that screening for sickle cell disease and for the trait should be carried out on every recruit, regardless of race; second, that it should be one part of a battery of tests for other genetic disorders; and, third, that potential risks associated with the trait are so small that there is no need to exclude carriers from any military duty except for piloting aircraft.

Federal, state and local genetic screening programmes designed to identify those who have sickle cell disease and those who carry the sickle cell trait have generated considerable controversy for several reasons. One complaint is that, since the disease is largely found among the black population, many screening programmes re-

quire the compulsory testing of blacks but not of whites. The disease and the trait can, however, show up among whites—particularly those of Mediterranean origin—and such screening programmes have been criticized on racist grounds. Another common complaint is that, although possession of the trait involves little or no hazard to the carrier, those who show up positive in the tests are being discriminated against, for example, in paying higher insurance premiums. Finally, it is often argued that, since the chief reason for screening for the sickle cell trait is to advise couples on the risks associated with having children, it serves little purpose to tell a child that he or she carries the trait.

Many of these considerations have been brought sharply into focus by a recent report on four military recruits who died during training at high altitudes for no apparent reason. Each was, however, subsequently found to be a carrier, and possession of the sickle cell trait has thus been implicated in their deaths. But the NAS committee found that the evidence to link possession of sickle cell trait with sudden death is entirely circumstantial. According to Dr Robert F. Murray, chairman of the committee, "it is guilt by association", and the report suggests that "there must be other, yet unknown contributory factors, not necessarily related to the sickle cell trait, that make some persons susceptible to collapse and/or sudden death". For this reason, the committee recommends that sickle cell trait carriers should be excluded only from serving as pilots, where they may be subjected to low pressure or other unusual conditions. But the committee also acknowledges that this recommendation is conservative.

vide a better data base from which to assess the potential capabilities of a network of seismometers around the world for policing a test ban, and in particular to try to ascertain how great the problem of false alarms—shallow focus earthquakes whose seismic characteristics look like explosions—will be. But the upgrading will also provide a welcome benefit for geophysical research, for the data from the upgraded stations will be available to the seismological community in digitized form so that they can be handled immediately by computers. At present, much time is taken up in transcribing film records from WWSSN stations into digital form. The higher quality data which are likely to come from the quieter sites and improved instrumentation will also be greeted with enthusiasm.

But will upgrading the WWSSN stations bring a comprehensive test ban treaty any closer? First, it is important to realize that the upgraded system is intended to be a research system, and not a surveillance system to police a test ban. Nevertheless, it could provide an important mass of data to help iron out some of the seismological problems that still remain in discriminating between small earthquakes and low yield explosions.

Briefly, both earthquakes and explosions produce a variety of seismic waves which travel through the body of the Earth (body waves) and along its surface (surface waves). Research aimed at discriminating between earthquakes and explosions has concentrated on measuring the proportion of energy from an event generated in the form of long period surface waves (M_s) and short period body waves (m_b). It turns out that, in general, earthquakes are more efficient generators of surface waves than are explosions, and the so-called $M_s:m_b$ discriminant is usually sufficient to tell them apart. The focal depth of the event is also a powerful discriminating factor, for those which take place deeper than a few kilometres can automatically be classed as earthquakes.

But there are a few earthquakes each year which for some reason do not generate the predicted proportion of surface waves, and they are liable to be incorrectly classified as explosions. The reasons for such anomalous earthquakes are not well understood, nor is

it possible to predict with certainty where such events may occur. More light may be shed on this problem by upgrading the WWSSN stations.

There is also the problem of the ever present background noise which limits the ability of seismometers to pick up signals from very small earthquakes or explosions. Seismometers installed at any site will record ground slippage and earth movements resulting from atmospheric conditions, traffic and other factors, and such interference can never be eliminated entirely. This has two important consequences. The first is that seismic signals from a very small explosion would be lost in the background noise, and a test ban treaty would have to be negotiated with this in mind, and the second is that it may be possible for a determined violator of such a treaty to use a variety of means for depressing the signals from larger explosions so that they too would be masked by the background noise, or hidden in the signals of large earthquakes.

All these considerations were brought out during hearings held in November 1971 by the Joint Committee on Atomic Energy (see *Nature*, **234**, 6; 1971). And at that time there was considerable discussion of the ultimate capability of a seismic network for detecting and discriminating between explosions and earthquakes. The estimates ranged between a detection threshold of about $m_b=4.0$ (equivalent to an explosion of one or two kilotons fired in solid rock) to about $m_b=4.75$. It was also made clear during the hearings that the problem of anomalous events increases markedly the smaller the event.

The joint committee itself, whose support will be a key factor in determining whether there will be negotiations for a test ban, was also concerned about the ways in which a determined violator could escape detection under a test ban. But at least one key member of the committee has become convinced that a test ban should now be negotiated, for Senator John Pastore, who was chairman of the joint committee in the 92nd Congress, is a co-sponsor of the resolution to be introduced into the Senate next week.

The chief stumbling block in negotiations for a comprehensive test ban has always been the insistence by the US government that seismic means alone are not sufficient to police such a treaty, and that on-site inspection is required as an ultimate assurance against violation, and the insistence by the Soviet government that on-site inspection is not needed. The official US position is unlikely to change until the Department of Defense is reasonably satisfied that a test ban could be adequately policed by seismometers, without a great false alarm problem, and

without too great a risk of evasion. Upgrading the WWSSN stations and deployment of new long period arrays could be a step towards that goal.

NOAA

False Promises

by our Washington Correspondent

ALTHOUGH the scientific community is by now attuned to the fact that political rhetoric is infrequently followed by hard cash, officials in the National Oceanic and Atmospheric Administration's earthquake and hurricane modification programmes are understandably feeling hard done by. Earthquake research and hurricane control, it should be remembered, are part of the Administration's much publicized drive to bring the forces of science and technology to bear on problems affecting society; and the 1973 budget, sent to Congress a year ago, promised large increases in funding for NOAA's work in these fields. Instead of largesse, however, many of the programmes have been given the axe.

In short, NOAA has been forced by financial stringencies to get out of the business of earthquake prediction, earthquake hazards assessment and earthquake engineering, give up its management of the National Earthquake Information Center and the Worldwide Standard Seismographic Network (WWSSN) and suspend the chief hurricane modification programme, Project Stormfury. Many of the earthquake programmes will be transferred from NOAA to the US Geological Survey in the Department of the Interior, but at the beginning of this week a plan for the transfer was awaiting approval by the Office of Management and Budget. The idea is to save money by consolidating NOAA's geophysics and geomagnetism operations with those of the USGS.

The plan for saving most of NOAA's seismology and geomagnetism facilities has been agreed to by officials of the National Science Foundation, the US Geological Survey and NOAA, but approval by the Office of Management and Budget is needed before it can be put into effect. In any case the transfer will probably not take place until the end of June. Essentially, the plan would leave NOAA with responsibility for only three seismic observatories needed for the tsunami warning system. Nearly all the rest of its seismology and geomagnetism operations would be transferred to the USGS and the National Science Foundation would provide some of the money for operating them. Since the Geological Survey also runs a large earthquake research programme, the idea would be to con-

Correction

In the note "Belt Tightening in Store for US Science" (*Nature*, **241**, 303; 1973), the figure of \$87 million in the last paragraph of column 1 on page 304 should have read \$87,000 million.

solidate facilities, and thus to save money.

Seismologists in universities will be anxious to see that data from seismological observatories will still be readily available under any new arrangements. This is what has tentatively been decided: the National Earthquake Information Center, which issues data on the preliminary location of epicentres of seismic events around the world, will be transferred to USGS, but more complete data will continue to be available from NOAA's Environmental Data Service. In other words, seismology data will be available from the same sources, but half will be managed by USGS and the rest by NOAA. Small wonder that one NOAA official described the whole situation this week as "a mess".

As for Project Stormfury, it is being "suspended" because NOAA does not have enough suitably equipped planes. NOAA was planning to move the project from the Atlantic, where it has had dubious success in mitigating the effects of hurricanes by seeding, to the Pacific. The activities of the National Hurricane Research Center will also be scaled down.

The consolidation of earthquake research within USGS is a casualty of President Nixon's cutback on federal expenditure this year, but it must also be acknowledged that there have been many complaints during the past few years that earthquake research is poorly coordinated in the federal government—the latest such complaint came from the General Accounting Office last year. Those who have argued for co-ordination will be the first to complain, however, if their recommendations are used as excuses for cutbacks.

INTERSTELLAR COMMUNICATION

Eye on the Future

by our Cosmology Correspondent

"We now have the technological capability of mounting an effective search for extraterrestrial intelligent life." This is the principal conclusion reached by B. M. Oliver after the Project Cyclops summer study programme, funded by NASA, of which he was co-director. The full report of that summer study has now been published, and provides perhaps the most detailed study of the interstellar communication problem now available (copies may be obtained from John Billingham, NASA/Ames Research Center, Code LT, Moffett Field, California 94035).

One-third of this 240-page report deals with the fundamental problem of life in the universe, sets out reasons for attempting communication and determines the most effective means of contact. Not surprisingly, it turns out that

electromagnetic radiation is the best tool for the job; the rest of the report, which specifies in considerable detail the antenna system needed, is where this study has the edge over previous work.

The size of instrument which the Cyclops team require is some 3 to 5 km diameter. They suggest that a circular complex of antennae, each 100 metres in diameter, surrounding a control room complex would be suitable for the job, the antennae being linked and phase switched to mimic one giant steerable array. This huge phased microwave antenna would be useful for many purposes; some idea of its power is given by the analogy that it could map Venus by radar as accurately as the Moon is mapped today, or Pluto with the accuracy now available for Mars studies. The advantages to radio astronomy are an obvious fringe benefit. An order of magnitude more powerful than existing arrays, the Cyclops system could also incorporate optical telescopes for simultaneous observation of sources. The sensitivity of Cyclops would be 4×10^6 times that of the late Ozma system (for a detailed comparison of Cyclops and Ozma see table) and could detect signals from thousands of light years away, assuming a plausible power output from the source.

Table 1 Comparison of Ozma and Cyclops Parameters

Parameter	Ozma	Cyclops
Antenna diameter	26 m	3,160 m
Antenna efficiency	0.5	0.8
System noise temperature	350 K	20 K
Resolved bandwidth	100 Hz	0.1 Hz
Integrating time	100 s	10 s
Instantaneous bandwidth	100 Hz	200 MHz

What would all this cost? According to the Cyclops study group, \$600 million per annum would allow preliminary planning and site development over 3 years followed by antenna construction at a rate of 100 per year. This compares not unfavourably with large projects such as Concorde, and although vastly greater than any previous scientific budget, is not entirely out of court.

Would the benefits be worth the cost? Certainly, such sums have been spent in the past, and are being spent today, on projects of little or no value to mankind. Even if the assumptions made in the study are completely correct, contact with a superior civilization would be unlikely for decades at least. J. C. G. Walker, writing in this issue of *Nature* (p. 379), is rather less optimistic. Although he agrees that such communication is technologically feasible, he believes that a successful search for extraterrestrial civilizations might require more than a thousand years, even if most habitable planets are

occupied by communicative civilizations.

Whether or not such contact is desirable remains largely a philosophical question, and the devotion of some detailed consideration to the use of Cyclops as a research tool could be regarded as an attempt by the team to justify its otherwise abstract project.

Certainly radio astronomy could benefit greatly by being given the funds for an order of magnitude improvement in observation instead of building more instruments comparable with those now in commission. The scale of expenditure suggested for Cyclops, however, runs at four times the total annual expenditure of the British Science Research Council. Perhaps the question is whether it is more efficient or more satisfying to spend \$600 million a year asking the stars for, say, a cure for cancer or to spend the same sum on cancer research on Earth.

Short Notes

RIP

AN item on page 1 of the January 31 issue of the *New York Times* invited readers to turn to page 38 for an article about how President Nixon is revamping "the entire top echelon of the federal science establishment". Page 38 was, appropriately, the obituaries page. (The article appeared on page 42.)

Kennedy Bill

SOME members of the Administration believe that the recent reorganization of the White House Policy machinery may have pulled the rug from under Senator Kennedy's National Science Policy and Priorities Act (S32). The first part of Kennedy's bill calls for the National Science Foundation to be given broader powers in formulating US science policy, and that, of course, is a central part of the reorganization plan announced last week. They also believe that the last part of the bill, calling for retraining programmes for unemployed scientists and engineers, will lose some political appeal if the projected improvement in the unemployment situation materializes. Discussion of the bill would then boil down to its provision for increasing the applied research and development activities of the National Science Foundation. But Congressional supporters of the legislation do not see it that way. For one thing, the bill's statement of objectives would require expenditures on civilian science to increase until they at least reach parity with expenditures on military science, and thereafter to grow at the same rate as the gross national product; the Administration's budget holds out little hope of such growth.

NEWS AND VIEWS

Explaining the Deuterium in the Universe

THE recent detection of deuterium in the Orion Nebula and near the galactic centre has stimulated the interest of astronomers in the processes by which this isotope is produced. Previously the presence of deuterium was definitely established only in the solar system; other measurements merely gave upper limits. Now it seems that the abundance of deuterium relative to hydrogen near the galactic centre is comparable with the terrestrial values, and an even higher deuterium abundance is deduced from the relative strengths of HCN and DCN lines in the Orion Nebula. It is conceivable that some fractionation of D and H occurs in molecule formation, in which case it is not impossible that the terrestrial value, $D/H \sim 10^{-4}$, is fairly widespread in the Galaxy.

Astronomers study the chemical composition of objects in the Galaxy with the hope of discovering to what extent their composition is characteristic of the Galaxy at birth and to what extent it is determined by nuclear reactions which have occurred during the galactic lifetime. It is known that nuclear reactions in stars gradually change the chemical composition of the stellar material and that, if stars lose mass into the interstellar medium, newly formed stars will have a chemical composition different from that of earlier generations of stars. Normal processes of stellar nucleosynthesis do not, however, produce deuterium, which is very easily destroyed by nuclear reactions at the high temperatures in stellar interiors.

It has been known for some time that a relatively smooth abundance of deuterium in the Galaxy could be explained if the life history of the Universe is correctly described by a Big Bang cosmological theory. According to this theory, in which the Universe was initially very hot and very dense and subsequently expanded and cooled, the chemical composition after the initial stages was essentially a mixture of hydrogen and helium and a very small amount of deuterium. The theory also predicts that the Universe should now be filled with microwave radiation which is a highly redshifted relic of the radiation present in the hot early stages of the expansion. The discovery of a cosmic microwave background with a temperature of 2.7 K stimulated interest in the Big Bang theory, which further predicts a relation between the present mean density of the Universe, the microwave temperature and the proportion of helium produced. Although the present mean density of the Universe is not known accurately, all expected values give an initial helium production which is approximately in agreement with observations. By contrast, a deuterium abundance comparable with the terrestrial value is only obtained for the lowest allowable values of the mean density of the Universe; for higher values less deuterium is produced.

If there proves to be a deuterium abundance throughout the Galaxy which is comparable with the terrestrial abundance, the simplest explanation is that the deuterium was produced in a Big Bang. It is not quite as simple

as this, for some deuterium must have been destroyed in matter that has passed through stars, but, given a model of galactic evolution, it should be possible to allow for this destruction. Even if it is plausible that the deuterium is *primaeval*, it is nevertheless necessary to ask whether there are any other processes that could produce a substantial amount of deuterium during the galactic lifetime.

There are two reasons for asking this question. The first is the obvious one that, if there are production processes as well as the destruction mechanisms just mentioned, they must both be studied along with the *primaeval* abundance in trying to understand the present observations. The second reason is that, although a Big Bang theory is capable of explaining the observed deuterium abundance, one is hesitant to believe that the mean density of the Universe is as low as this would require. To obtain the required *primaeval* helium abundance almost all the matter in the Universe has to be the matter which is known to be contained in galaxies, so that the intergalactic medium must be essentially non-existent. Although there is still no sure detection of a general intergalactic medium there are several pointers to its existence. For example, attempts to understand the formation of galaxies are much more difficult if it is demanded that most of the matter in the Universe went into galaxies than if the intergalactic medium is still the principal form of mass in the Universe.

For these reasons, Hoyle and Fowler consider on page 384 of this issue of *Nature* whether substantial amounts of deuterium can have been produced in the galactic lifetime. Although deuterium is destroyed efficiently by thermonuclear reactions inside stars, there is one obvious way in which it can be produced. This is by breakdown or spallation nuclear reactions in which α particles are hit by fast protons moving at a significant fraction of the speed of light. Various authors, including Hoyle and Fowler, have previously suggested that, as a result of such reactions during the formation of the solar system, the terrestrial value of D/H might be higher than the initial value for the solar system. Hoyle and Fowler now suggest that these spallation reactions may have occurred in outgoing shock waves from a supernova explosion, or as the result of the explosion of a more massive object, and they estimate that there may have been sufficient explosions to give a general galactic abundance of deuterium comparable with the abundance in the solar system.

It is clear that much more work needs to be done before the relative importance of *primaeval* and galactic production of deuterium is certain. It is also obvious that there will be continued interest in the subject that Hoyle and Fowler have previously named "deuteronomy".—R. J. T.

Modulation of Transmitter Release

ALTHOUGH it is clear that intercellular communication in the nervous system generally involves the release of chemical transmitter substances from nerve terminals, the mechanisms which may exist for modulating the process of chemical transmission are still obscure. In the past two years, however, several findings have been reported which suggest how the amount of transmitter release at nerve terminals in response to the arrival of nerve impulses may be regulated by the state of ongoing activity at such terminals. In particular, two mechanisms have been proposed to account for the fall-off in the release of noradrenaline from adrenergic nerve terminals during periods of repetitive stimulation.

The first of these mechanisms, proposed by Hedqvist (*Acta Physiol. Scand. Suppl.*, 345; 1970), suggests that the release of noradrenaline is controlled by a negative feedback system which involves a local synthesis of prostaglandin E. Sympathetic nerve stimulation or the addition of exogenous noradrenaline has been shown to stimulate the production and release of prostaglandin E from various adrenergically innervated tissues. Furthermore, the exposure of these tissues to exogenous prostaglandin E severely depresses noradrenaline release from the adrenergic terminals, whereas the addition of drugs

known to inhibit prostaglandin E synthesis enhances the neurally evoked release of noradrenaline.

A second feedback mechanism, supported by several recent findings (Enero *et al.*, *Brit. J. Pharmacol.*, **44**, 672; 1972; N. S. Starke, *Archiv. Pharmacol.*, **274**, 18; 1972), suggests that noradrenaline may inhibit its own release by an action on α -adrenergic receptors, probably located on the surface of the presynaptic adrenergic terminals. This suggestion is supported by the finding that a variety of α -adrenergic receptor stimulating drugs depress the release of noradrenaline from adrenergic terminals in the heart and other tissues. Conversely, α -adrenergic receptor blocking drugs, such as phenoxybenzamine or phentolamine, enhance the release of noradrenaline from sympathetic terminals in response to nerve stimulation. Both proposed mechanisms thus suggest that the release of noradrenaline from adrenergic terminals is a self-limiting process, which is controlled by local negative feedback mechanisms. Whether those mechanisms are independent, or coupled parts of a single control system, however, has not been clear.

Two new reports throw new light on these problems. Stjärne (*Nature New Biology*, **241**, 190; 1973) puts forward evidence which suggests that both of the braking mechanisms described earlier may co-exist in the adrenergic terminals of guinea-pig vas deferens. He studied the stimulus-evoked release of radioactivity from the adrenergic terminals of the isolated vas deferens prelabelled by exposure to radioactive noradrenaline and found that the addition of an inhibitor of prostaglandin synthesis enhances noradrenaline release, whereas low concentrations of prostaglandin E inhibit release. Addition of the α -receptor blocking agent phentolamine, however, after inhibition of prostaglandin synthesis causes a further large increase in noradrenaline release; this suggests the removal of a second braking system capable of operating even when prostaglandin synthesis is inhibited. The α -receptor stimulating drug methoxamine can also inhibit noradrenaline release, after inhibition of prostaglandin synthesis. These results suggest that noradrenaline release, at least from the terminals in guinea-pig vas deferens, is controlled by two different braking systems. Furthermore, these systems do not seem to be interrelated, but to be mediated by different basic mechanisms.

The findings reported by Szerb and Somogyi (*Nature New Biology*, **241**, 121; 1973), furthermore, suggest that an "auto-inhibition" mechanism for modulating the amount of transmitter released from nerve terminals may also exist in cholinergic neurones. These authors monitored the release of acetylcholine from slices of rat cerebral cortex exposed to electrical field stimulation, by measuring the outflow of radioactivity from slices preloaded by incubation with tritium-labelled choline. With this recently developed technique acetylcholine release from cortical slices can be measured *in vitro* in the absence of a cholinesterase inhibitor, usually added to prevent the hydrolysis of released acetylcholine. Szerb and Somogyi found that the evoked release of acetylcholine from such slices is inhibited by the addition of an inhibitor of acetylcholinesterase, and a similar inhibi-

Beetles and Archaeology

BEETLES, it seems, can tell one more about ecological conditions in late glacial and post-glacial times than any other group of fossil land animals, and their Quaternary fossil record can be linked very closely with ecological and distributional knowledge of the species today. This was first clearly demonstrated in Britain through the pioneer researches of G. R. Coope, R. G. Pearson and others. Recent archaeological investigations of a site near Doncaster (see page 405 of this issue of *Nature*) indicate that Bronze Age men of about 1,100 BC made extensive clearances in a major oak forest, in which occurred a number of species of beetles no longer part of the British fauna.

These findings of Buckland and Kenward give some hint of the extent to which the original forest fauna of post-glacial Britain has been impoverished through human clearances and exploitation of low-land woods. Such fragments as still remain of deciduous woods, for example, in parts of the New Forest and of Sherwood Forest, are themselves gravely threatened with further faunal impoverishment—there is not one nature reserve in the Sherwood Forest area, and even Denny Wood in the New Forest, which is a nature reserve, has recently been subject to considerable felling in the service of interests other than those of nature conservation. Many more species of woodland beetles, which might provide valuable archaeological evidence, are in imminent danger of extinction in such places.

—From a Correspondent.

tion of release is produced by adding oxotremorine, a stimulant of cholinergic muscarinic receptors. The inhibition of acetylcholine release in either case is abolished by the cholinergic muscarinic receptor antagonist drug atropine, which by itself does not alter acetylcholine release. The authors suggest that acetylcholine may act to inhibit its own release by way of muscarinic receptors, and that several previous reports that atropine can cause a stimulation of acetylcholine release can probably be explained by the fact that cholinesterase inhibitors were used in these experiments. In such conditions the high concentration of acetylcholine accumulating in the extracellular space may have caused an artefactual inhibition of transmitter release.

There have been many reports that the rate of release or turnover of the monoamine transmitters noradrenaline, dopamine and 5-hydroxytryptamine in the mammalian central nervous system is generally lower in response to the administration of receptor stimulating drugs and increases in response to receptor blocking drugs. Such findings have been described by the generic term "receptor feedback", and have generally been interpreted in terms of complex polysynaptic reflex pathways. The present findings suggest that some of these phenomena may bear re-examination in the light of the possible existence of local "auto-inhibition" or "prostaglandin" feedback controls at the various different types of presynaptic terminal.—From our *Neuropharmacology* Correspondent.

Gamma-ray Statistics

ONE needs a certain grim determination to be a gamma-ray astronomer these days. It is now fourteen years since experimentalists were fired with enthusiasm by a paper by Morrison (*Nuovo Cim.*, **7**, 858; 1958) which stressed the importance of making astronomical observations in the gamma-ray region of the spectrum; but, in spite of the expenditure of a great deal of money and effort, there have been very few discoveries in gamma-ray astronomy.

The potential importance of this branch of astronomy is easy to understand. Observations in radio astronomy have shown that many regions of the Universe contain large fluxes of very energetic particles. The physical conditions in these regions are still poorly understood, but there is little doubt that the energetic particles should produce gamma-rays in interactions with matter, with radiation and with magnetic fields. Understanding of these regions should, therefore, benefit greatly from observations in the gamma-ray part of the spectrum.

Several basic experimental problems have hindered the development of gamma-ray astronomy. The Earth's atmosphere strongly absorbs all but the highest energy gamma-rays and the measurements must be made from high altitude balloons or from satellites. There is also a large background flux of gamma-rays produced in interactions of cosmic-ray particles with the Earth's atmosphere or with the detector itself. Finally, the flux of gamma-rays which can be expected from even the strongest sources is very small. This means that statistical fluctuations seriously limit the sensitivity of detectors.

The development of the spark chamber in the 1960s gave a fresh impetus to gamma-ray astronomy. At ener-

gies greater than 10 MeV gamma-rays interact with a detector through the process of pair production, and the direction of the electron-positron pair—and hence the direction of the gamma-ray—can be measured accurately in a spark chamber. During the 1960s, larger and larger spark chambers were built for experiments on high altitude balloons; the biggest now has an area of 2,500 cm² and the balloons provide exposure times of the order of a day. In 1972 the SAS-B satellite, containing a spark chamber with a collecting area of 225 cm², was launched.

An alternative experimental approach is possible for gamma-rays with energies greater than 10⁶ MeV. When a gamma-ray of this energy interacts with the Earth's atmosphere it creates an electromagnetic cascade of electrons, positrons and gamma-rays. The electrons and positrons produce Čerenkov radiation in the atmosphere, and this reaches the ground as a pool of light several hundred metres in diameter. This light can be detected, and its direction can be measured by means of a photomultiplier at a focus of a concave mirror. The effective area of this detecting system is the area of the pool of Čerenkov light, which is very much larger than the area of the mirror, and this helps to compensate for the very low number of gamma-rays which can be expected at these energies. This technique has been fully exploited by the group at the Smithsonian Observatory, led by Fazio and Weekes, which has constructed a concave reflecting system 10 m in diameter at Mt Hopkins, Arizona.

The results of all this experimental effort have been disappointing. It was soon realized that the early theoretical predictions were far too optimistic; in fact, gamma-ray astronomers still do not know how sensitive their detectors should be if they are to develop this branch of astronomy. Of course, the past fourteen years have not been uneventful. Whenever any band of faithful devotees search doggedly for the truth, some will see hallucinations. In gamma-ray astronomy hallucinations take the form of statistical analyses, carelessly applied. One may not agree with Ehrenfest, who is reputed to have told a student, "If you need to use statistics to support your argument, then you are probably wrong anyway"; but one seems to read too often of results which "... have a very low probability of being due to a chance fluctuation" but, eventually, turn out to be just that.

On page 376 of this issue of *Nature* O'Mongain attempts to apply rigorous statistical tests to data published in recent experiments. He concludes that, at an energy of approximately 100 MeV, four sources have been detected—the pulsar in the Crab nebula, two sources in Sagittarius and one in Libra. At energies greater than 10⁶ MeV only the Crab pulsar has been detected. No doubt, some experimentalists will disagree with O'Mongain's analysis; but few will deny that the subject benefits from an occasional critical survey of this kind.

This year will be a crucial one for gamma-ray astronomy. The SAS-B satellite was launched late in 1972 and, apparently, is functioning well. By the end of the year there may well be many more well authenticated sources. The experimental problems will still be severe because astronomy is concerned not only with detecting sources but also with measuring their size and position, measuring spectra and detecting time variations, all of which will require detectors with even greater sensitivities. But when one has cleared Beecher's first time round, the rest of the course looks much less formidable.—R. R. H.

RADIATION

Tissues at Risk

from a Correspondent

SUBJECTS discussed at a meeting held in London on January 9 by the Society for Radiological Protection included malignancies arising from exposure of the bone and lung to radiation and recent data which have led to improved methods of assessing the radiation dose to those tissues from radionuclides deposited therein.

In his account of the origin, structure and radiosensitive areas of the bone, Dr J. F. Loutit (MRC Radiobiology Unit, Harwell) identified haematopoietic marrow, endosteum and cranial epithelia adherent to bone surfaces as the principal sites containing cells with a high proliferative potential and therefore at risk from radiation. He added that malignancies can and do arise elsewhere and cited as an example a high incidence of haemangioendotheliomas—tumours of the walls of the blood vessels—which he had recently observed in mice injected with ^{90}Sr . The induction of tumours is related not only to the chemical behaviour and energy range of the radiation of bone seeking radionuclides but also to half life. Whereas ^{224}Ra with a short 3.8 day half life induces in man chiefly osteosarcomas, ^{226}Ra with a half life of 1,620 years induces both osteo and fibrosarcomas.

The relative radiotoxicities of ^{224}Ra and ^{226}Ra were considered by Dr G. W. Dolphin (National Radiological Protection Board, Harwell) who pointed out that per unit dose averaged over the skeleton, ^{224}Ra , the shorter lived nuclide, induces a greater number of tumours than does ^{226}Ra . One reason for this difference could be that the dose to cells at the bone surface relative to that averaged throughout the skeleton will be greater for ^{224}Ra than ^{226}Ra because the short-lived ^{224}Ra releases much of its α energy at the bone surface and ^{226}Ra most of its α energy after it has become distributed throughout the bone volume.

Methods developed by Professor F. W. Spiers (University of Leeds) for calculating the microdistribution of dose within the tissues of bone were described by Miss J. Whitwell (University of Leeds) and Mr P. Darley (Central Electricity Generating Board, Berkeley). These methods have already assisted greatly in the interpretation of the effects seen from radionuclides deposited in bone.

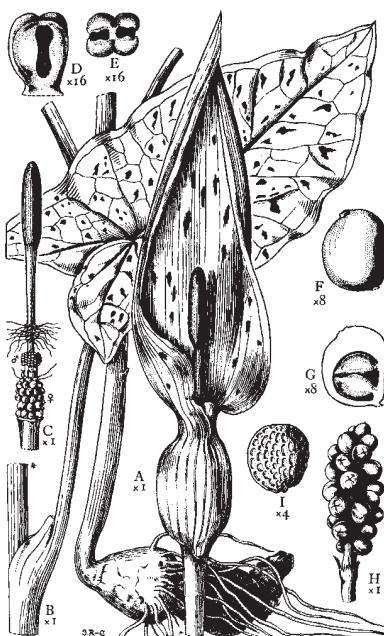
Dame Janet Vaughan (University of Oxford) identified the haematopoietic marrow as the tissue at greatest risk when the whole body or trunk is exposed to external radiation. The most common malignancy is leukaemia, but, as Dr Vaughan emphasized, the latent

period for leukaemia is short relative to that for other neoplasms such as osteosarcoma. These have been observed in groups which have been studied for 10 years or more after exposure. Such people are the ankylosing spondylitics and the children originally irradiated *in utero*. Leukaemia attributed to bomb radiations has been more in evidence in Hiroshima than in Nagasaki. Fast neutrons present in Hiroshima but not in Nagasaki have been incriminated. One effect of localized external irradiation in young children has been the development of exostoses, benign bony outgrowths, rather than osteosarcomas after radiation therapy for enlargement of the thymus.

In her review of the structure and metabolism of the lung Professor L. Reid (Institute of Diseases of the Chest, London) described as recent findings the presence of nerve endings in the alveoli and in the epithelium of the airways and the function of the brush borders in regulating the amount of fluid on the surface of the airways. She also defined the areas of deposition of particles of various sizes and also their clearance times. The longest, 60 to 120 days, is the normal clearance time from the alveoli, but this can be increased by tobacco smoke which immobilizes the cilia in the airways.

In spite of difficulties in obtaining relevant information concerning the incidence of radiation-induced lung cancer, Dr S. Rae (National Radiological Protection Board, Harwell) said that it is higher than normal in groups of haematite, fluospar and uranium miners exposed to radon. Cancers are predominantly of small undifferentiated cells in the hilar region of the lung and are more numerous on the right than on the left side. Some of the cancers may be attributable to the daughter products of radon.

BRITISH PLANTS

Lords and Ladies

THIS figure shows Stella Ross-Craig's drawing of *Arum maculatum* (commonly known as lords and ladies, cuckoo pint or wake robin) for the thirtieth and penultimate part of her authoritative series *Drawings of British Plants* (Bell, London, 1973). This new section covers the Juncaceae, Typhaceae, Sparganiaceae and Araceae.

Dr J. Vennart (MRC Radiobiology Unit, Harwell) said that the present recommendations of the International Commission on Radiological Protection in respect of maximum permissible concentrations were based on radiation

Nuclear Steroid-receptor Complex

ENDOCRINOLOGISTS nowadays believe that steroid hormones are transported to the nuclei of target cells by specific cytoplasmic receptors which bind the hormone, presumably at the cell surface, and then migrate to the nucleus. Precisely how the hormone enters the nucleus and there alters the pattern of gene expression remains obscure, but, according to Liang and Tymoczko who write in *Nature New Biology* next week (February 14), the nuclei of prostate and uterine cells contain ribonucleoprotein particles respectively able to associate specifically with androgen and oestrogen-receptor complexes.

Liang and Tymoczko prepared ^3H -hormone-receptor complexes from cytoplasmic extracts of rat prostate and calf uterine tissue. They also isolated from

the chromatin of the nuclei of these cells ribonucleoprotein particles which sediment at 30S and 80S respectively. *In vitro* these nuclear ribonucleoprotein particles specifically associate with the corresponding ^3H -hormone-receptor complexes. After heating or treatment with pancreatic ribonuclease, however, the nuclear particles lose this ability to bind hormone-receptor complex.

Liang and Tymoczko speculate that these nuclear particles may play some part in either effecting the entry of the hormone-receptor complex into the nucleus or mediating the action of the hormone on the genome. Whether these speculations are valid will no doubt emerge as these nuclear ribonucleoprotein particles are further characterized.

dose averaged throughout the whole lung, presumably because the actual sites of critical tissues had not yet been identified. A comprehensive model for deposition and clearance of dust from the airways was now available and could be used to calculate doses in different parts of the respiratory system, for example, to the cells of the bronchial epithelium, which is a possible site for the induction of cancer by radon. It has been suggested that the lymph nodes may not be critical sites because observed malignancies in experimental animals are largely pulmonary. There is a need to identify the radiation sensitive tissues and to examine the ICRP concept of dose commitment which implies that risk is related to the radiation dose integrated over 50 years without allowance for the latent period between induction and manifestation of cancer and for the lifetime of the critical cells.

The practical implications in respect of emergency reference levels of radiation following nuclear emergencies were outlined by Miss P. M. Bryant (National Radiological Protection Board, Harwell). Because of the present greater understanding of both the tissues at risk and the metabolism of radionuclides such as ^{90}Sr there is a possibility that these levels may be increased.

ENTOMOLOGY

Emergence Times

from our Insect Physiology Correspondent

It has long been known that the eclosion—emergence—of different insects tends to occur at a specific time of the day or night, and that individuals not quite ready to achieve eclosion one day must wait until the same time next day. That was described many years ago by Corbet for emerging dragonflies. In the current lingo an act or process controlled in time by a circadian clock of this kind is termed a "gated event", on the helpful analogy of a gate which is opened only at fixed times of day and for fixed periods. The emergence of *Drosophila* from the pupa is a good example described by Pittendrigh and Skopik in 1970.

There are several examples of the gating process being centred in the brain. That is so in the photoperiodic termination of pupal diapause in *Antheraea* (Williams and Adkisson, 1964). And in this same silk moth, Truman and Riddiford (1970) showed that under the action of the appropriate photoperiod certain neurosecretory cells in the brain liberate an "eclosion hormone" which switches the entire behaviour from pupal to adult type. This includes release of the labial gland secretion which dissolves the silk cocoon, and all the other acts involved

in escaping from the old cuticle, expansion of the wings and lysis of the inter-segmental muscles. Secretion of the eclosion hormone is evoked by the photoperiod to which the pupa is exposed; but liberation of the hormone at a particular time of day is gated by an inherent circadian rhythm in the brain.

In a recent report Truman (*J. Exp. Biol.*, 57, 805; 1972) describes a study of the corresponding process of moulting during the larval stages and shows that the timing of the moults is not controlled by a similar mechanism. A direct correlation exists between the time of ecdysis and the time of release of the prothoracotropic hormone (PTTH) from the brain, which does not suggest that ecdysis is controlled by a circadian timing mechanism. The time of ecdysis is sensitive to temperature, which suggests that a developmental process is involved. Moreover the synchrony of ecdysis within a group of larvae is maintained even in larvae ligated at the neck: the head is not needed to trigger ecdysis. In fact, the time of shedding the larval cuticle is determined by a developmental process which requires a fixed number of hours (depending upon temperature) after the gated release of the PTTH from the brain. For this process a longer time is required in *Antheraea* as the larva increases in size; thus each successive ecdysis occurs at a progressively later time of day.

Reversion of HGPRT⁻ Cells

MUTANT cells resistant to particular drugs, coupled with cell fusion techniques, are being routinely used today by somatic cell geneticists to produce hybrid cells for genetic analysis. In particular, mutant mouse A9 cells and mouse RAG cells which lack hypoxanthine-guanine phosphoribosyl transferase activity (HGPRT⁻) and so are resistant to 8-azaguanine have been widely used in fusion experiments with cells lacking thymidine kinase (TK⁻) activity and are therefore resistant to bromodeoxyuridine. When HGPRT⁻ cells are fused to TK⁻ cells the hybrids can be selected by plating in Littlefield's HAT medium in which the parental cells cannot grow but the hybrids survive because of complementation. But, as Shin, Caneva, Schildkraut, Klinger and Siniscalco point out in *Nature New Biology* next week (February 14), this selection procedure rests on the assumption that the mutant parental cells do not revert, and they find, contrary to expectation, that both A9 and RAG mutants do indeed revert from the HGPRT⁻ phenotype to the HGPRT⁺ phenotype at a low but significant rate.

The nature of the stimuli that induce secretion of the PTTH, which acts on the prothoracic gland to cause the release of the moulting hormone, ecdysone, is not known in the case of caterpillars. But once these stimuli have been received the brain releases hormone from the neurosecretory cells only during a specific time of day—the "gating period". Although the timing and duration of these gating periods, during which hormone is released, are more or less independent of temperature, the percentage of the population which is able to exploit a given gate, being dependent on a developmental process, is markedly influenced by temperature. It is interesting to find these two different mechanisms for the timing of ecdysis operating respectively in the caterpillar and in the chrysalis of the same insect.

PROTEINS

Innards in situ

from our Molecular Biology Correspondent

SOME interesting new offerings in the current journals concern themselves with aspects of the chemical and conformational constraints that act on a polypeptide segment and its side chains when it forms part of a native globular protein. How, for instance, to assess the effect of a unique environment within a protein on the behaviour of an ionizing group is a problem that has

Because reversion of these mutant A9 and RAG cells had never been reported, Caneva *et al.* chose them for an attempt to detect transformation of mammalian cells. They exposed A9 and RAG cells to HeLa cell DNA in the presence of inactivated Sendai virus to facilitate uptake of the DNA and they also set up the appropriate control experiments. Much to their surprise mouse cells able to grow in HAT medium and containing HGPRT activity could be selected from both experimental and control experiments.

Moreover, electrophoretic analysis of the HGPRT in these cells revealed that the enzyme was murine, not human. Apparently, therefore, in conditions which promote cell fusion, A9 cells and RAG cells revert at rates of 1.8×10^{-8} and 9.0×10^{-8} respectively. Why this should be the case when the cells do not revert if plated in HAT medium in the absence of Sendai virus and the nature of the genetic changes in the parental "mutant" cells and the revertants are intriguing problems. Clearly these findings have far reaching implications for somatic cell geneticists who are using these particular cells.

long exercised protein chemists. Spectroscopic and chemical methods have been devised, and one of the most successful has recently sprung from the discovery that the C-2 ring protons of histidine side chains can be resolved in proton magnetic resonance spectra, so that in favourable cases the titration curves of the individual histidine of a protein can be determined.

An altogether different and potentially very useful approach is given a preliminary airing by Matsuo *et al.* (*J. Biochem., Tokyo*, **72**, 1057; 1972), who report as follows: the isotope exchange rate of the C-2 imidazole ring proton can be observed by NMR, and follows the pK curve of the ring protonation. Thus a plot of the deuteration rate constant against the pH gives the imidazole titration curve. Now at acid pH , the rate is evidently infinitesimally slow; thus the exchange rate against tritium can equally well be followed by quenching the reaction at various times with acid, and then determining the degree of titration by scintillation counting. The pK of histidine in a tripeptide and of the single histidine residue in lysozyme were determined in this way. After acidifying, the lysozyme solution is passed through a column, and this is followed by acid hydrolysis paper electrophoresis of the product and scintillation counting of the basic amino-acid fraction. Evidently the imidazole side chain is the only one in the protein that retains its tritium through the hydrolysis. The pK of his-15 turns out to be pH 5.2, whereas in the denatured state it is found to be 7.0. The possibilities will no doubt not be lost on protein chemists, for if this method can be made to work equally well in other situations, and combined with enzymatic hydrolysis and peptide separation, all manner of interesting cases, involving histidines in active sites and the like, could be explored.

The steric accessibility of side chains in proteins is commonly judged by their ability to react with specific reagents. Perham (*Biochem. J.*, **131**, 119; 1973) advocates the use of the chromophoric imidoester, methyl picolinimidate, for the determination of available amino-groups in proteins. In TMV protein, the two lysines (53 and 68) react, whereas in the intact virus lys-53 becomes unavailable. In mutants with extra lysines the same situation recurs: a lysine at position 140 remains reactive in the intact virus. In a set of two mutants a lysine at position 9 reacts, whereas one at 33 is reactive in the monomeric protein, but is evidently engulfed in the structure of the whole virus. There is independent evidence from the position of a heavy-atom label in crystals that position 140 is indeed on the outside of the particle. Position 9 is also expected to occur on the surface,

for the first nine residues make up a highly polar segment. At residue 10 hydrophobic character sets in, and the side chains presumably submerge in the structure. In the one mutant it therefore seems as though two out of four lysines, in the intact virus at least, are sequestered. From an energetic viewpoint it should be advantageous if they were to emerge in ion-pair interaction with carboxylate groups. Until comparisons with known X-ray-based structures can be made, the structural implications of anomalous reactivity of side

chains must, of course, be approached with caution, as Perham is at pains to stress.

At the conformational level, a new method, minted and burnished in Anfinsen's laboratory, is now on display (Sachs *et al.*, *Proc. US Nat. Acad. Sci.*, **69**, 3790; 1972). This depends on the preparation of two kinds of antibodies to a given determinant, or group of determinants, on a protein. One results from immunization with peptide fragments, the other with the whole protein, a specific population of antibodies being

Reiteration of Duck Haemoglobin Genes

In the hands of biochemists reverse transcriptase purified from RNA tumour virus particles has become an invaluable tool for the synthesis of single-stranded DNA complementary to cellular messenger RNAs. The DNA made by reverse transcriptase can be labelled to high specific activities, and for this reason it can be used as a probe for messenger RNAs and their DNA templates in hybridization experiments designed, for example, to measure the expression of a particular gene or the number of copies of that gene per genome. Using this approach Bishop and Rosbash, as they report in *Nature New Biology* next Wednesday (February 14), estimate that in duck cell there may be only two or three copies of haemoglobin genes.

Bishop and Rosbash isolated 10S RNA containing globin messenger from immature duck red blood cells and in the presence of oligo (dT), which acts as a primer for reverse transcriptase, used this RNA as a template for the

synthesis of short strands of complementary DNA (cDNA) with a molecular weight of about $1-2 \times 10^5$. They then used this cDNA to isolate a cDNA-duck cell DNA duplex. Measurements of the rate of reannealing of cDNA-duck DNA and of duck DNA in the presence and absence of cDNA were then made. From the results of these C_0t analyses, and making allowances for the various problems in interpreting such data, Bishop and Rosbash estimate that the reiteration frequencies of duck haemoglobin genes may be as low as two to three.

This estimate is in close agreement with that reported by Bishop *et al.* last year from data obtained from RNA-DNA hybridization experiments; they concluded that the reiteration frequency of haemoglobin genes in ducks was probably less than five. Such a low degree of reiteration is, of course, in marked contrast to estimates that in sea urchins there may be about 1,200 copies of histone genes.

α Globin mRNA is Lacking in α Thalassaemia

In next Wednesday's *Nature New Biology* (February 14), Grossbard, Terada, Dow and Bank demonstrate that the decreased production of structurally normal α globin chains, typical of α thalassaemia syndromes, can be localized to the level of production of α chain messenger RNA. These workers have investigated the synthesis of α and β globin chains in reticulocytes from non-thalassaemic blood and those from the blood of a patient with haemoglobin H (HbH) disease. The red cells in HbH disease contain a tetramer of four β chains as a result of the relative deficiency of α chain synthesis.

Intact cells from HbH and non-thalassaemic patients were incubated with radioactive leucine, the cells were lysed, globin was prepared and the α and β chains were separated on carboxy-methyl-cellulose (CMC). The ratio of α/β chain synthesis in the in-

tact cells of the patient with HbH disease was 0.55, and the α/β ratio in non-thalassaemic cells was 1.0. Next, mRNA was isolated from HbH and non-thalassaemic blood, and was assayed in a cell-free system from Krebs ascites tumour cells. The products of the cell-free systems were analysed by CMC chromatography and this yielded α/β ratios of 0.79–1.0 for non-thalassaemic cells and 0.13 and 0.17 for HbH cells. The low α/β ratios in HbH blood were not dependent on the concentration of mRNA used.

Thus the decreased production of α globin chains in α thalassaemia is reflected in the chain mRNA associated with polyribosomes. The mRNA in thalassaemic cells, therefore, is either decreased in amount or is abnormal. A similar situation of decreased globin mRNA activity has been reported previously for β thalassaemia.

then absorbed out of the antiserum on a column of a covalently immobilized peptide fragment. This antibody fraction binds specifically to the given peptide fragment, as well as to the intact protein. The differences between the two kinds of antibodies manifest themselves in a number of ways, and the implication is that they are specific respectively for the native and disordered conformations of the antigenic fragment, in this case residues (99–149) of Staphylococcal nuclease. This is the C-terminal part of the chain, and is known from the X-ray structure to be highly α -helical in the native protein, but essentially a random coil in isolation. The native conformation of the nuclease is stabilized by the ligands, calcium and thymidine diphosphate; now whereas these species have only a small stimulatory effect on the precipitin curve of nuclease with native-state antibodies to the single segment, they induce strong inhibition of the interaction with random-state antibodies. Moreover, the former causes inhibition of nucleolytic activity of the enzyme, whereas the other has no such effect.

Contrariwise, interaction of native-state antibody with the free fragment was weak (the binding measurements being made by estimation of uncombined antibody in terms of its inhibitory effect on the activity of added native nuclease). An active enzyme can be made by recombination of N-terminal and C-terminal fragments, but Anfinsen and his colleagues have also found that fragment (6–43) will combine with (50–149) to give what seems to be a conformationally native molecule, with, however, no activity (for which some at least of the missing residues are essential). This, so to speak, emasculated enzyme will combine well with the native-state antibodies. If then one regards the separated fragments of the chain, which, as stated, are more or less random coils, as existing in a state of equilibrium between the random and the folded conformations, in which the former is overwhelmingly favoured, then introduction of the native-state antibody will trap the folded form, and so displace the equilibrium.

From a comparison of the antibody-antigen association constant with the antigen in the folded and unfolded forms (intact protein and free peptide respectively), the conformational equilibrium constant for the peptide fragment can in principle be easily enough determined. Many other well characterized situations involving coupled equilibrium of this kind are known. This approach offers what is probably the only rational basis at present available for some sort of experimental estimate of a conformational equilibrium constant, when the equilibrium favours one form to an immoderate degree.

HUBBLE CONSTANT

New Determination

from a Correspondent

SINCE 1939 when Rudolph Minkowski used the 100-inch telescope on Mount Wilson to obtain slit spectra of supernovae, their interpretation has been a major puzzle to astrophysicists. Minkowski himself described the spectra as being overlapping Doppler-broadened emission lines of unknown origin. Recently, however, the notion originally due to Pskovskii has been gathering support that many, if not all, the spectral features can be explained as very broad absorption lines superposed on a continuous spectrum.

This interpretation has now been applied to Type I supernovae by David Branch and Bruce Patchett of the Royal Greenwich Observatory in the *Monthly Notices of the Royal Astronomical Society* (161, 71; 1972). Hitherto the Type I spectra have been the most puzzling of supernovae spectra as they lack even the familiar landmark of the Balmer lines of hydrogen which are present in Type II supernovae. Branch and Patchett show that one can identify nearly all the intensity minima in the spectra of the two supernovae originally studied in the 1930s by Minkowski with lines of singly ionized iron with a blue-shift of 10,000 km s⁻¹. Further, they construct synthetic spectra using lines of titanium, scandium, magnesium and silicon as well as iron. These show that, when the excitation state and abundance of each species are considered, the iron lines are indeed most prominent, that several remaining minima in the spectra are explained and that the overall agree-

ment of the observed and synthetic spectra is good.

Branch and Patchett then go on to use their line identifications and the outflow velocity derived therefrom to determine the distance to the supernovae by the first application of a method originally suggested by Leonard Searle of the Hale Observatories. Because by the Stephan-Boltzmann law the luminosity of the supernova is proportional to the product of the area of the star and the fourth power of the effective surface temperature, data on the light and colour variations of the supernova as the outburst proceeds give the fractional rate of increase of radius with time. Fixing the outflow velocity leads therefore to a value for the luminosity and hence distance of the supernovae. Although no light and colour curves exist for the supernovae studied by Minkowski, Type I supernovae are sufficiently homogenous to adopt mean curves for the class which are good enough to give an absolute magnitude $M_v \sim -20.8 \pm 0.8$.

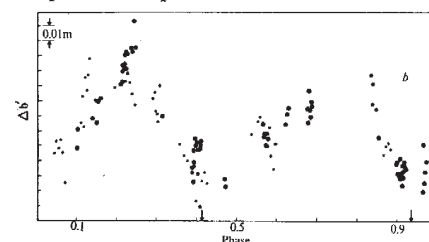
The real importance of this result is that the absolute magnitudes of such distant objects, in galaxies with appreciable cosmological recession velocities and beyond the effect of any local irregularities, provide a new method of determining the Hubble constant. This method is quite independent of the normal astronomical methods which rely on a step-by-step process of distance determination from nearby stars by way of a series of "standard candles" (variable stars, globular clusters, and so on) whereas the method used by Branch and Patchett is, if their model is correct, more directly related to the laws of

Optical Studies of Cyg X-1 (HDE 226868)

THE intriguing system Cyg X-1 is presently receiving attention from X-ray, optical and radio astronomers. The X-ray source was identified with the star HDE 226868 because of the coincidence of a radio outburst from that star with changes in the X-ray flux from Cyg X-1 (see *Nature*, 241, 246; 1972). The properties of that star have suggested to some astronomers that the X-ray emission must be coming from a black hole orbiting the BO supergiant; but the issue is still open.

In next Monday's *Nature Physical Science* (February 12), Lester, Nolt and Radostitz report photometric monitoring of HDE 226868 (see diagram). These observations do not show the same variability of hydrogen emission which others have reported (see, for example, Bolton, *Nature*, 235, 271; 1972) and in addition there is now some doubt about whether or not there is an eclipse of the associated X-ray source at all.

Lester *et al.* suggest that the evidence now favours one of two models. First, the companion may not be the source of X-ray emission, or the X-ray emitting region may not be eclipsed; second, the optical data at least are consistent with a tidally distorted binary with no optical eclipse (a possibility also considered by Bolton, *Nature Physical Science*, 240, 124; 1972). They do not come down on either side of the fence, and it looks as if Cyg X-1 will continue to present a puzzle.



Observations of HD 226868, b' filter measurements plotted against phase. Arrows indicate conjunction.

physics. It is thus pleasing that their result of $H_0 = 40^{+25}_{-13}$ km s⁻¹ Mpc⁻¹ is in good agreement with recent determinations by more traditional methods.

EXOTIC NUCLEI

Limits to Growth

from a Correspondent

THE problem of determining the limits of nuclear stability is one of the great challenges in nuclear physics at present, both from a theoretical and an experimental standpoint. It is, however, a problem that is proving difficult to solve experimentally for all save the lightest nuclei, such as the isotopes of beryllium and boron. It is now known, for example, that ¹²Be is stable against neutron emission. On the other hand ¹³Be and ¹⁴Be are likely to decay in times of order 10⁻²³ s by neutron emission, and cannot be regarded as stable. Recently much effort has been invested in developing a formula which will give the masses of all nuclei in the periodic table, from which the stability of any nucleus can be predicted.

One global approach invokes a liquid drop theory, but for nuclei far from stability the level of accuracy is of the order of several MeV, which is also likely to be the order of the binding energy of neutron-rich nuclei. A more accurate technique uses a stratagem relating the existing mass differences of known nuclei to predict masses of new nuclei to an accuracy of 200 keV. This is the basis of the Garvey-Kelson mass formula, which predicts the stability of such neutron-excessive nuclei as ²⁸O (8 protons, 20 neutrons). Other theories predict the limit at ²⁴O, and it is now the task of experimenters to produce these nuclei in order to make a systematic check on the various theories. Ultimately this problem is of fundamental interest to nuclear physics, because it will be important to see if theories of nuclear structure (such as the shell model) are at all reliable when applied far from the regions of stability in which they were developed.

A dramatic advance in this field was made recently by Klapisch and his group of the Laboratoire Rene Bernas at Orsay (*Phys. Rev. Lett.*, **29**, 1254; 1972); they have produced new isotopes of Na and K. The Garvey-Kelson method predicts that the Na isotope (11 protons) with neutron number between 20 and 28 will be stable if the number of neutrons is even, and marginally unstable if odd. Thus ³¹Na, ³³Na, ³⁵Na and so on should be stable. The Orsay group produced the new isotopes by bombarding uranium with 24 GeV protons from the CERN proton synchrotron. Under this bombardment many nuclear species are produced by spallation reactions, but the alkalis, in-

cluding isotopes of lithium, sodium and potassium, possess the peculiar and incompletely understood property that they will diffuse out rapidly from the heated graphite which surrounded the uranium target. They can then be transported between the beam bursts of the synchrotron to a detector, shielded by a cadmium enclosure to reduce the intense neutron background. The Na and K ions were selected for transmission to the detector by a simple mass spectrometer. The instrument was first set to detect an abundant isotope ²³Na, which has a long half-life, and then the voltage and fields on the spectrometer were accurately adjusted to search for new isotopes. Previous studies using this technique have shown the stability of Na isotopes out to ³¹Na (11 protons, 20 neutrons). Neutron number 20 is a "magic number" and completes a closed shell. It was possible that the extra binding of the shell closure would mark the limit of stability.

In the new experiment, the stability of ³²Na was clearly demonstrated, but the production cross-section was a factor of twenty smaller than for ³¹Na. This made the task of hunting for ³³Na rather difficult, but the researchers were also able to provide conclusive evidence for the stability of this isotope, with a

half-life between 5 and 35 ms. The stability of ³²Na was unexpected, because it marks a departure from the systematics of the Garvey-Kelson predictions, which seem to be over-pessimistic. The stability of ³³Na follows because the added pairing energy of the extra neutron leads to greater binding. It is almost certain that ³⁴Na and ³⁶Na will also turn out to be stable. An exciting possibility is the extension of the technique to study these highly neutron-excessive isotopes, because the decrease in yield from ³²Na to ³³Na is only a factor of five compared with twenty from ³¹Na to ³²Na. The dramatic drop in yield at neutron number 21 seems to be connected with the closure of the shell at 20 neutrons.

This experimental technique seems to hold the greatest immediate hope for advancing the limit in production of neutron-excessive light nuclei. Ultimately the technique will be able to give precise information on nuclear masses for a more detailed comparison with theoretical predictions. Another exciting possibility is that the method can be applied to a mass measurement of ¹¹Li. This nucleus is known to be stable, but almost every theoretical calculation predicts that it should be unstable to neutron emission by several MeV.

Extensional Viscosity Measured with Three Jets

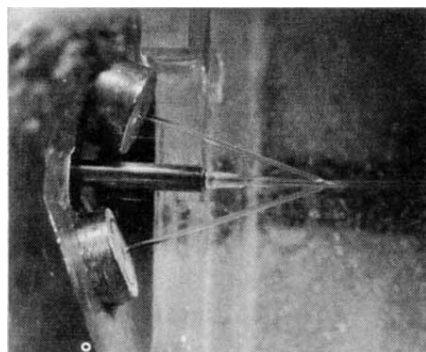
A NEW way of measuring the extensional viscosity of a liquid (that is the ratio of the time average stress to the extensional strain rate in a jet of the liquid) is reported in next Monday's *Nature Physical Science* (February 12) by Bragg and Oliver. Their apparatus is shown in the diagram; in this so-called "triple jet" system the two outer jets, in which the liquid is travelling at a high velocity, converge on the middle, slower-moving jet and stretch it.

The results and the technique are of particular interest in the context of certain industrial processes, fibre spinning for example, in which stretching flow plays a central part.

Bragg and Oliver were able to obtain extensional strain rates of between 100 and 800 s⁻¹, by comparison with other methods which have either had rather low strain rates (<1 s⁻¹) associated with them, or very high ones (~10⁴ s⁻¹). In the triple jet method the other component parameter in the extensional viscosity, the time average stress, is deduced by measuring the change in force on the central nozzle as the central jet is stretched.

As expected Bragg and Oliver found that the solutions they used—two aqueous solutions of polyacrylamide and one of 'Polyox'—were very resistant to stretching. Solutions of this type

that have shear viscosities of a few cpoise may have extensional viscosities of more than 10 poise; that is the Trouton ratio (of extension viscosity to shear viscosity) is often greater than 1,000. The Trouton ratio is three for a Newtonian fluid.



The triple jet arrangement in operation.

The results show that the extensional viscosity for the three solutions tested goes up slowly with strain rate. The 'Polyox' solutions seem to behave quite differently from the polyacrylamide solutions; the former have higher Trouton ratios at lower extensional strain rates. Thus at 488 s⁻¹ the Trouton ratio for 0.1 per cent polyacrylamide solution is 284, whereas at 131 s⁻¹ the value for 0.1 per cent 'Polyox' solution is 760.

The Eruption on Heimaey, Iceland

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Members of the University of Iceland describe the recent volcanic activity in Iceland and present analyses of some of the samples recovered.

ON May 5, 1967, the Surtsey eruption came to an end, having then lasted, with short intervals of quiescence, three and a half years. Nearly six years later, on January 23, 1973, a new eruption started in the Vestmannaeyjar archipelago, this time on the biggest island, Heimaey. On that island (see Fig. 1) is situated the town of Vestmannaeyjar; it has 5,300 inhabitants and is Iceland's most important fishing centre.

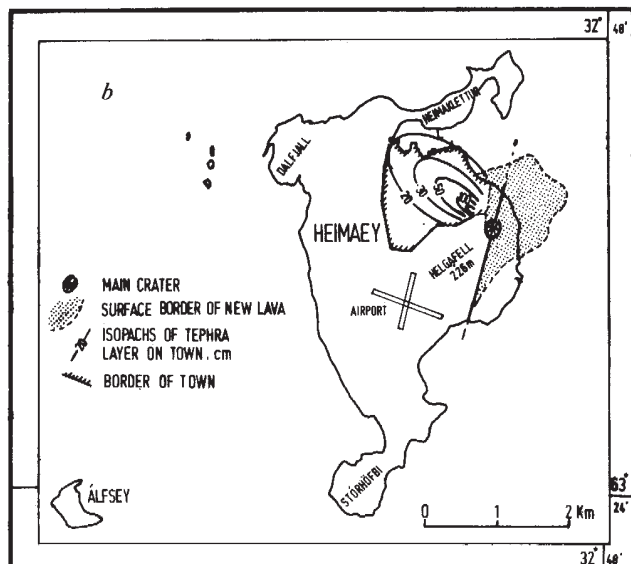
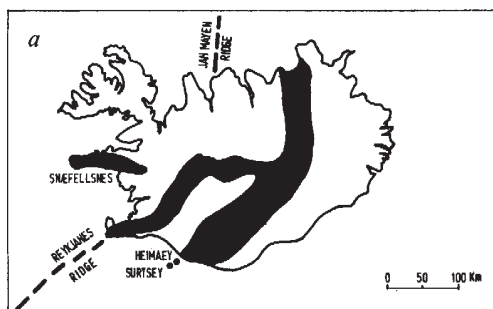


Fig. 1 *a*, The neovolcanic areas of Iceland; *b*, the eruption on Heimaey—preliminary map of the situation after five days of activity.

Precursory Symptoms

Between 2200 h GMT on January 21 and noon the following day many small earthquakes were registered by seismographs situated on the mainland of Iceland. None was felt by people on Heimaey. The foci seemed to be south of Heimaey. At 2230 h on January 22, tremors started again and at least three shocks were felt by people in the town of Vestmannaeyjar. They were more shallow and their foci were on Heimaey.

Eruption Begins

Between 0150 and 0155 h on January 23 a fissure began to open up on the east side of the island of Heimaey. About 5 min before the eruption started there were no signs of eruption here. Within a few minutes of the beginning of the eruption lava fountains were playing on a fissure 300 to 400 m long, and when the photograph in Fig. 2 was taken at 0438 h the length of the fissure was about 1,500 m. It was glowing along its entire length and the height of the glowing fountains was 50 to 150 m. The direction of the fissure is north-north-east to south-south-west.

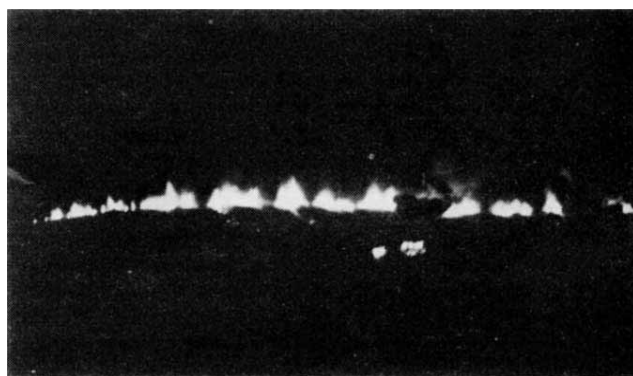


Fig. 2 The lava fissure viewed from the east at 0438 h on January 23. Length of fissure about 1,500 m. The two bright spots beneath the fissure are the two principal lava streams falling over a cliff into the sea.

As the most north-north-easterly part of the fissure runs only 200 to 300 m from the easternmost part of the town, evacuation of the inhabitants started immediately and within 4 h all inhabitants, except a few hundred who had to stay at their posts, had been evacuated without any casualties, chiefly by the fishing fleet which happened to be in the harbour but also by planes sent from the airports at Reykjavík and Keflavík. Because of its close vicinity to the eruption the town was threatened by tephra fall, principally glowing bombs, and by lava flow, the chief threat of which was that it would block the harbour. Fortunately the lava flowed principally towards the east and during the first few days the

wind blew the tephra cloud away from the town. But during the night of January 26, wind from the south-east blew the tephra over the town for 8 h. This happened again on January 27, between 1500 h and 0100 h the following night, and the isopachs on Fig. 1 show the thickness of the tephra layer in town after the latter tephra fall had ceased. Repeated south-easterly winds have now added to the thickness and on January 29 the thickness of the layer varied from 30 cm in the north-west part of the town to more than 5 m in the south-east part; between 50 and 100 houses have either been burnt down, chiefly by glowing bombs falling through windows, or buried by tephra, or they have collapsed because of the tephra load on their roofs. The windows facing the eruptions in the southern part of the town have now been covered with sheets of corrugated iron and this has greatly reduced the risk of house fires.

Continuing Activity

At 1030 h on January 23, the north-north-east end of the fissure had extended 150 to 300 m into the sea and small cock's-tail explosions built up a tiny island, which was soon reached and covered by the advancing lava. At noon on the same day the activity had nearly ceased in the southernmost third of the fissure and the fountain activity was beginning to concentrate in the middle of the fissure, where it had first opened up.

After five days of activity the lava flow (see Fig. 1) covered 1.35 km² and its volume was roughly 35 million m³, corresponding to an average flow of about 80 m³ s⁻¹. The flow during the first few days probably exceeded 100 m³ s⁻¹. The eruption column occasionally rose to 9,000 m, or nearly to the tropopause. The relative height of the principal tephra cone which formed around the active part of the fissure was about 100 m on January 27, and had increased to about 150 m (absolute height 180 m) by January 30.

On the morning of January 25, a short submarine ridge was built up to a height of about 10 m on the continuation of the fissure towards the north-north-east, nearly 1 km from its end. At the same time the fissure was extended about 300 m into the sea towards the south-south-west (Fig. 1), thus reaching a total length of nearly 3 km. Some explosions were observed at that end of the fissure, but since noon that day no submarine activity has been observed. Between January 26 and 30 the eruption slowed down considerably.

Ground Water Temperature

In 1964 a drillhole 1,565 m deep was sunk by the National Energy Authority at the west end of the harbour, some 2 km from the fissure. The day after the eruption started the water level in the hole had risen by 10 m, and the temperature at a depth of 800 m had increased by 10° C. Further developments are being monitored by the Energy Authority.

Chemistry and Petrography

Six new chemical analyses are reported in Table 1. The composition of the new rocks is intermediate between those Snaefellsnes rocks termed hawaiiite and mugearite by Sigurdsson¹. They belong to the alkali basalt-trachyte suite. The analyses represent four specimens from the first two days of the present eruption, and two from older formations on Heimaey. The samples were selected partly to test variation with rate and environment of cooling. One represents a lava flow, two are bombs, and one is a tephra. The chief difference between the four is, as to be expected, in the oxidation state of the iron; this aspect will be discussed later in this article.

Fluorine and chlorine were analysed in the leachate from

the scoraceous surface of sample 2 (No. 2a, see Table 2), resulting in 100 p.p.m. F and 190 p.p.m. Cl. Two of us (N. O. and S. S., to be published) found, first, that comparable samples from the Hekla eruption of 1970 contained 375 p.p.m. F and 200 p.p.m. Cl, and, second, that these elements are confined to the surface of the ejecta. Hence their concentration varies inversely with the grain size of the tephra. Thus, the Hekla tephra contained 1,000 p.p.m. F and 1,400 p.p.m. Cl at a distance of 170 km from the volcano.

Modal analyses of four samples representing different rates of cooling are shown in Table 3. Sample 8 is from a bomb ejected from the subaqueous crater at the northern end of

Table 1 New Chemical Analyses and Norms of Six Samples from Heimaey

Sample	1	2	3	4	5	6
SiO ₂	50.29	49.98	50.19	49.99	46.40	47.75
Al ₂ O ₃	16.77	16.96	17.45	17.26	14.31	16.45
TiO ₂	2.58	2.53	2.32	2.40	1.65	3.16
Fe ₂ O ₃	2.15	2.14	1.55	1.17	1.77	2.33
FeO	10.57	10.64	11.07	11.36	10.21	10.93
MnO	0.34	0.33	0.34	0.33	0.22	0.28
MgO	3.22	3.18	3.08	3.12	10.61	4.75
CaO	6.99	7.07	7.08	7.18	11.75	8.84
Na ₂ O	4.95	4.88	4.84	4.89	2.49	4.05
K ₂ O	1.30	1.35	1.29	1.36	0.32	0.86
P ₂ O ₅	0.71	0.68	0.62	0.70	0.17	0.42
H ₂ O ⁺	0.12	0.04	0.02	0.02	0.05	0.03
H ₂ O ⁻	0.05	—	—	—	0.05	0.07
Sum	100.04	99.78	99.85	99.78	100.00	99.92
CIPW norm						
QZ	0.00	0.00	0.00	0.00	0.00	0.00
OR	7.68	7.97	7.62	8.03	1.89	5.08
AB	39.54	38.14	37.93	36.49	16.50	30.52
AN	19.69	20.38	22.07	21.12	26.92	24.16
LC	0.00	0.00	0.00	0.00	0.00	0.00
NE	1.26	1.70	1.63	2.64	2.47	2.02
CO	0.00	0.00	0.00	0.00	0.00	0.00
WO	4.50	4.45	3.91	4.32	12.67	7.18
EN	1.67	1.62	1.31	1.42	7.65	3.33
FS	2.91	2.92	2.72	3.03	4.33	3.78
EN	0.00	0.00	0.00	0.00	0.00	0.00
FS	0.00	0.00	0.00	0.00	0.00	0.00
FO	4.42	4.38	4.42	4.42	13.08	5.92
FA	8.57	8.72	10.14	10.44	8.21	7.46
MT	3.11	3.10	2.24	1.69	2.56	3.37
IM	4.90	4.80	4.40	4.55	3.13	6.00
HM	0.00	0.00	0.00	0.00	0.00	0.00
AP	1.55	1.49	1.35	1.53	0.37	0.92
RT	0.00	0.00	0.00	0.00	0.00	0.00
Water	0.16	0.03	0.01	0.01	0.09	0.09
	100.02	99.76	99.83	99.76	99.93	99.89

Analyst: Science Institute, Division of Geosciences, University of Iceland.

For explanation of sample numbers see Table 2.

the fissure. The bomb was chilled in the sea. The modes, however, indicate but a minor increase in crystallization from the chilled sample to a lava. The bomb is highly vesicular, and optical examination reveals that the crystals were present in the melt before vesiculation, for feldspar laths may be seen protruding into vesicles and sidling alongside them. An inevitable conclusion is that the magma contained some 47% of crystals at the time of eruption. This is in agreement with the high viscosity observed, and the rather low temperatures measured so far in the molten Heimaey lava (1,030° C by comparison with 1,140° C in Surtsey²).

The refractive index of the glassy groundmass was measured in four samples. In samples 1 and 2 $n_{\text{glass}}=1.572$; in tephra erupted on January 23 at the south end of the fissure $n_g=1.568$, and in tephra erupted on January 24 at the north end of the fissure $n_g=1.575$. Clearly, in view of the advanced degree of crystallization even of chilled samples the refractive indices should not relate to the chemical analyses.

The minerals present are plagioclase, olivine and titanomagnetite. Titanomagnetite was the first phase to crystallize. It occurs as inclusions in both the other minerals, and forms skeletal microphenocrysts about 0.05 mm across. Curie-point analyses on unoxidized samples give $T_c = 80^\circ\text{C}$, corresponding to 70% of ulvöspinel in the titanomagnetite.

Table 2 Glossary of Samples Described

Sample No.	Sample description	Collected by
1	Tephra erupted 0400 h, January 23, 1973	S. Jakobsson; January 23, 1973
2	Core of lava spatter erupted 1100 h, January 23, 1973	S. Steinthórsson; January 23, 1973
2a	Margin of same	S. Steinthórsson; January 23, 1973
3	Bomb erupted 1100 h, January 23, 1973 (VE 110)	S. Jakobsson; January 23, 1973
4	Lava at southern end of fissure, flowed January 24, 1973 (VE 111)	S. Jakobsson; January 24, 1973
5	Prehistoric lava from Stórhöfði, Heimaey (VE 61)	S. Jakobsson; 1966, 1967
6	Volcanic plug, Dalfjall, Heimaey (VE 93)	S. Jakobsson; 1966, 1967
7	Lava from south end of fissure, flowed January 23, 1973	H. Kristmannsdóttir; January 24, 1973
8	Centre of bomb ejected from subaqueous crater at north end of fissure 0200 h, January 23, 1973	Th. Einarsson; January 23, 1973
8a	Margin of same	Th. Einarsson; January 23, 1973

The modes in Table 3 seem to indicate that plagioclase crystallized next. The volume of olivine increases from 3.7% in the chilled sample to 6 or 7% in the more crystalline ones, whereas in the same samples the amount of plagioclase only increases from 34 to 36%. The plagioclase forms laths ranging in size from 0.2×0.04 to $0.5 \times 0.1\text{ mm}^2$. Larger phenocrysts (some 2 mm across) occur very rarely. One such has been isolated so far and awaits microprobe analysis. The groundmass plagioclase shows somewhat undulatory extinction, but is not distinctly zoned. In sample 7 the composition, as determined with the U-stage, is $\text{An}_{45} \pm 2$, and in sample 8 compositions range from An_{45} to An_{55} .

Table 3 Modal Analyses (Volume %)

Sample No.	Plagioclase	Olivine	Opakes	Glassy groundmass
3	36.0	7.6	6.0	50.4
4	37.5	5.3	5.8	51.4
7	35.4	6.1	5.4	53.1
8	33.9	3.7	6.3	56.1

Olivine forms euhedral, frequently diamond-shaped crystals, about 0.15×0.1 to $0.35 \times 0.1\text{ mm}^2$ in size. Lath-shaped crystals also occur and their size is about $0.15 \times 0.05\text{ mm}^2$. Inclusions of glass are very common, and they have roughly the same shape as the host crystal. The $2V_x$ was estimated to be 70° , from which a composition of Fo_{50} to Fo_{60} is deduced.

Cooling and Oxidation

On the basis of Curie-point measurements (made by Mr Leó Kristjánsson) of samples 1 to 4 their oxidation states may be divided into three groups: unoxidized lava (No. 4) shows a clear-cut Curie temperature of about 80°C , indicat-

Table 4 Ferric-ferrous Ratios and Curie Temperatures of Some Samples

Sample No.	1	2	2a	3	4	8	8a
$\text{Fe}_2\text{O}_3/\text{FeO}$	0.203	0.201	0.603	0.140	0.103	0.128	0.232
$T_c^\circ\text{C}$	560	80/560	80/560	80	—	—	—

ing titanomagnetite rich in ulvöspinel. In this sample microphenocrysts of homogenous titanomagnetite predominate. Tephra (No. 1) has a distinct Curie point at 560°C . This high degree of oxidation must be ascribed to the influence of the atmosphere during cooling. In the volcanic bombs (Nos. 2 and 3) the curve of demagnetization with temperature possesses a shoulder, such that the upper portion extrapolates to about 80 to 100°C on the temperature axis and the lower portion to 560°C . In the bombs the outer rim is oxidized whereas the core is unoxidized. This may be clearly seen in polished samples: near the margin the titanomagnetite

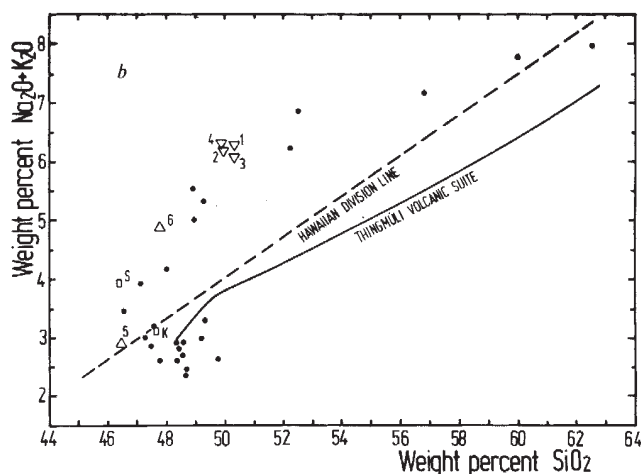
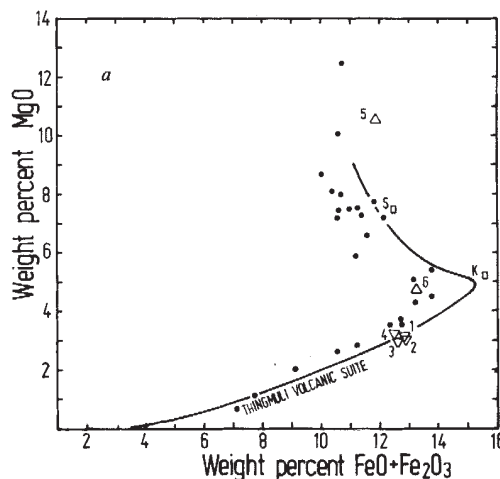


Fig. 3 Analyses of the two alkalic areas. ∇ , Heimaey 1973; $\triangle$, older formations on Heimaey; $\square$, Surtsey 1963 (S) and Katla 1918 (K); $\bullet$, postglacial lavas on Snaefellsnes.

microphenocrysts show a high degree of oxidation (oxidation index III) and the groundmass is studded with tiny grains of magnetite. The surface, and the insides of vesicles close to the surface, are characterized by haematite. Within these samples, oxyexsolution of the titanomagnetite microphenocrysts ceases, and the magnetite disappears from the groundmass. Thus, phreatic activity leads to oxidation, and lavas

should be more oxidized than pahoehoe because of their larger surface area. The rapidity of oxidation may be gauged by the fact that in the water-chilled bomb (No. 8) the outer portion is considerably more oxidized than the core. In Table 4 the ratios $\text{Fe}_2\text{O}_3/\text{FeO}$ of the analysed samples are compared with the Curie temperatures when available. This process of surface oxidation is not to be confused with the increase in oxidation state inwards from crust to core in pillows, dikes and lava flows. The latter phenomenon is a consequence of the buildup of volatiles trapped within the rock, whereas the former is caused by oxidation in the atmosphere.

Pleistocene to 1,500 or 2,000 yr BP (ref. 5). Conversely, the eastern volcanic zone is now the most active area in Iceland. The greater part of the Vestmannaeyjar islands is thought to have been formed during a relatively short time span about 5,500 yr ago (ref. 6 and S. P. Jakobsson, to be published). This shift in activity from Snaefellsnes to the eastern zone can be related to tectonic changes in Iceland during the past 3 or 4 m.y. (ref. 7 and K. Saemundsson, to be published) in response to westward drift of the North Atlantic system of plates relative to the Icelandic hot spot.

The alkalic areas are situated on the flanks of the actively spreading rift system. The depths of the 2/3 and 3/4 seismic

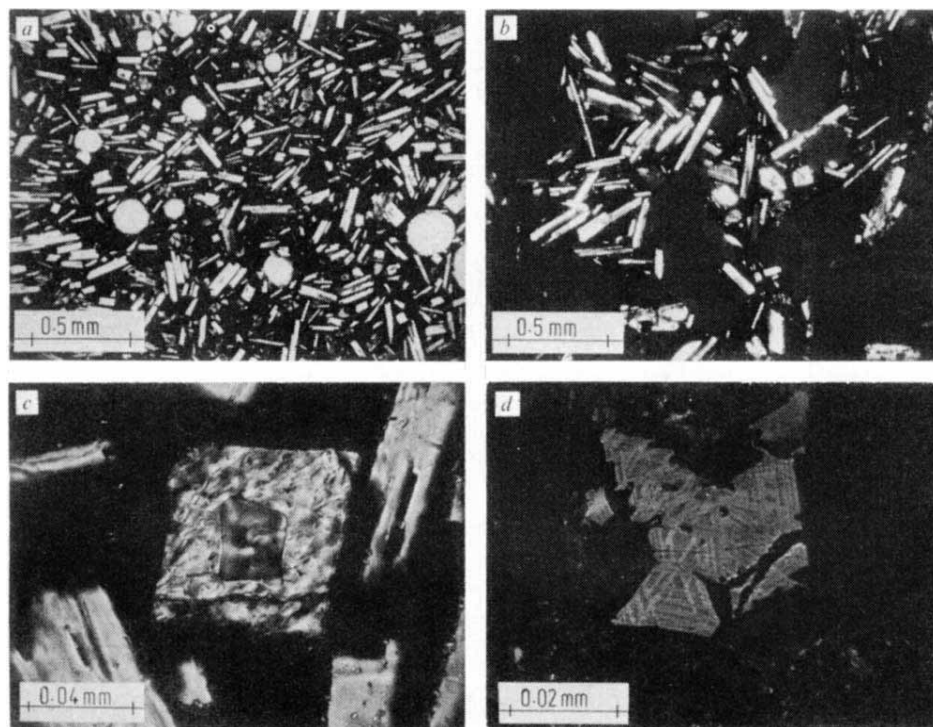


Fig. 4 Photomicrographs of some of the samples. *a*, Lava flow (No. 7). Note alignment of plagioclase laths and inclusions of glass in olivine crystals. (Plane polarized light.) *b*, Chilled bomb (No. 8). Note relationship between plagioclase laths and vesicles. (Crossed nicols.) *c*, Olivine crystal with inclusion of glass. From lava flow No. 7. (Plane polarized light.) *d*, Oxidized titanomagnetite microphenocryst showing trellis exsolution of ilmenite (oxidation index III). From oxidized part of lava spatter (No. 2a). (Reflected plane polarized light; oil immersion).

Photomicrographs of some of the samples are shown in Fig. 4.

Implications

Recent volcanics in Iceland seem to fit into a distinct pattern of distribution³. Alkali basalts are confined to the Snaefellsnes peninsula and the south end of the eastern volcanic zone. In particular, the lavas of the central volcano Snaefellsjökull, at the extreme end of the Snaefellsnes peninsula, provide the most complete recent sequence of alkali basalts and their derivatives in Iceland (S. S., to be published). There, the volumes of individual hawaiitic and mugearitic lavas are frequently of the order of 0.1 km³. At the time of writing (January 30) the volume of the new Heimaey rocks is estimated to be about 0.04 km³.

The Vestmannaeyjar region (S. Jakobsson, to be published) shows a more restricted range of compositions. The present Heimaey volcanics are the most differentiated rocks so far analysed from that suite (see Fig. 3 where analyses of the two alkalic areas are plotted). The Thingmuli tholeiitic volcanic suite is shown for comparison⁴.

The Snaefellsnes area seems to be all but extinct. The last eruption in that region probably occurred about 1,000 yr ago. Snaefellsjökull itself probably ranges in age from the late

boundaries in Iceland increase away from the Reykjanes zone to the west and east⁸ with a concomitant decrease in geothermal gradient⁷. Thus the alkalic provinces may be related to a greater depth of fusion for these rocks, by contrast with the tholeiitic composition of the ridge volcanics⁹.

Note added in proof. Microprobe analysis (performed at the Smithsonian Institution, Washington, DC, by courtesy of Dr T. Simkin) has yielded the following results. Bomb ejected on January 23: groundmass plagioclase is $\text{An}_{53.6}\text{Ab}_{46.4}$ and olivine is $\text{Fo}_{61.6}\text{Fa}_{38.4}$. Tephra erupted at 1400 h on January 26: plagioclase is $\text{An}_{55.2}\text{Ab}_{44.8}$ and olivine is $\text{Fo}_{63.7}\text{Fa}_{36.3}$. Plagioclase and olivine compositions range over 5 and 2%, respectively.

¹ Sigurdsson, H., thesis, University of Durham (1970).

² Sigurgeirsson, Th., *Surtsey Research Progress Rept.*, Nos. I, II, III (1965, 1966, 1967).

³ Jakobsson, S. P., *Lithos*, **5**, 365 (1972).

⁴ Carmichael, I. S. E., *J. Petrol.*, **5**, 435 (1964).

⁵ Steinthórsson, S., *Náttúrufr.*, **37**, 236 (1968).

⁶ Kjartansson, G., *Náttúrufr.*, **36**, 136 (1966).

⁷ Pálmason, G., mimeographed report (National Energy Authority, Iceland, 1972).

⁸ Pálmason, G., *Soc. Sci. Isl.*, **40**, 187 (1971).

⁹ McBirney, A. R., and Gass, I. G., *Earth Planet. Sci. Lett.*, **2**, 265 (1967).

Application of Statistics to Results in Gamma Ray Astronomy

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Some of the published results in gamma ray astronomy are examined, and it is concluded that not all the sources which have been mentioned can be confidently considered to be present.

THE development of detection techniques in gamma ray astronomy above 1 MeV has begun to yield new and exciting results over more than six decades of energy. The small flux of photons at these energies and the limited collection areas available mean that most of the data consist of small numbers. Observers have usually realized the importance of statistical fluctuations when dealing with these small numbers, so that when there occur positive fluctuations considerably in excess of those expected statistically, it is not unreasonable to ascribe these effects to a "source".

It has already been seen from one set of observations on the Crab Nebula pulsar¹ and the statistical notes it prompted^{2,3} that great care must be exercised when expected fluctuations are calculated; full allowances must be made for all degrees of selection imposed on the data. Other apparent effects have appeared⁴⁻⁶ that have not been verified in later experiments⁷⁻⁹, although they were deemed statistically unlikely to be chance fluctuations. In this article, a more rigorous application of statistical methods is proposed, along with the use of the relative likelihood function as a source discriminator. The use of the latter function has been described most clearly for these applications by Hearn¹⁰ and by Fichtel *et al.*¹¹ for upper limit determinations.

The Poisson probability $P(N) = \frac{B^N}{N!} e^{-B}$ expresses the

probability that an event occurs N times in a large group of trials when the average number of occurrences is B , the true average number of occurrences, obtained from an infinite set of trials. The Poisson distribution is discrete, that is, all N are positive integers, and B is a positive number. One often wishes to test whether an experimentally determined number N is likely under two assumptions: (a) that the probability distribution of the experimental results is Poissonian, and (b) that the mean measured level is indeed equal to B .

Thus, for n sample values of B ($B_1, B_2, B_3, \dots, B_n$) determined experimentally, one should test whether this sample is drawn from some Poisson distribution with an unknown real

mean B . The best estimate of B is $B' = \frac{1}{n} \sum_{i=1}^n B_i = \bar{B}_n$, that

is, the arithmetic mean (ref. 12, p. 435). The distribution of B_i should be tested by the χ^2 test for agreement with the Poisson distribution (ref. 12, p. 436). Unless this agreement is good, no physical conclusions can be drawn from a single measurement that disagrees with this distribution. Even when the distribution of B_n is found to agree with the Poisson distribution, the value of the mean $B' = \bar{B}_n$ is still only an estimate of the real mean B . The sample size for B' determination should be so large, and the variance of B' so small, that little uncertainty is introduced into any calculations of probability.

In practice, the number of sample measurements of B_i is often so small that (a) the uncertainty in the estimate of B is large and (b) agreement with Poisson distribution is assumed from experimental experience, rather than tested. Under these circumstances, application of a Poisson probability test must take into account the uncertainty in the estimate of B . Thus, for any single measurement N , a range of possible values of B will exist, with a related range of probability levels.

Even after these tests and allowances have been made, a single experimental determination of N may be found to be highly improbable, for either or both of the following reasons: (a) the background level B fluctuated randomly, although this is unlikely for this measurement, or (b) an extra source contribution S was present, that is, $N \sim S + B$. Hearn¹⁰ has shown most clearly how the relative likelihood method can be used to determine, at any given confidence level, whether (b) is relatively more likely than (a). The relative likelihood method can take into account the Poisson nature of the measurements while upper limits, flux confidence levels, and flux errors are obtained in a consistent way. Simultaneously, the sometimes physically unrealistic results of dealing with "standard deviations" can be avoided. Nonetheless, the same limitations apply in this case; the likelihood value is still dependent on a realistic determination of B .

As Hearn has pointed out, the confidence that one can place in any measurement N being the result of a flux S is reduced as the number of attempts to find a large enough value of N increases. Thus, if M bins in the sky are searched for a likely flux S , the confidence one can place in any source found is reduced by a factor M . Equally, for M possible bins in a pulsar-phase analysis, or M possible regroupings of the data, any confidence level is reduced by M . The number M should include all choices of data selection, grouping, regrouping, and reanalysis. It is often very difficult to estimate the number of subjective choices that are made in grouping data; M is only equal to unity when there is only one physically realistic grouping of the data attempted, and only one bin in this grouping in which an effect will be tested for and accepted.

To determine if the results of any search for a gamma ray effect can, in fact, be ascribed with a given degree of certainty to a source, rather than to a statistical fluctuation, one needs to know N , the number of events observed; B , the number of events expected; E_B , the standard error on the estimate of B ; and M , the number of attempts made to find N . In the tests that follow, attempts are made to calculate realistic likelihood levels for some of the published gamma ray results; for comparison, Poisson probability levels are also calculated. In many cases, E_B or M are not available in the published results. For these cases, and the others used here, the tests are intended to show only that some of the apparently large fluctuations may not be as improbable as claimed. The tests represent a statistical demonstration rather than a critical analysis of results. I am responsible for any misrepresentation of data that may be present. The results tested are grouped into regions that correspond to those used in ref. 13. Each group of results is analysed by using the formulae and notation of Hearn¹⁰: these are described in the first group that follows, but the original article should be consulted for complete definitions.

Galactic Centre Region

Table 1 shows the results of tests on observations of the Galactic centre region. For the results of the London group¹⁴, a particular known source position Sgr XR-2 was tested with M assumed equal to 1. Following Hearn, the relative likelihood of the fluctuation observed being due to a source contribution S is

$$L(S) = \frac{P(S/N)}{P(S=0/N)}$$

where the P s are conditional probabilities. For the case of Poisson probabilities, this simplifies to

$$L(S) = (S/B + 1)^N e^{-S}$$

and the likelihood value L quoted is the maximum value of $L(S)$. The maximum of $L(S)$ is at $S = S_m = N - B$ (for $N \geq B$); $L = L(S_m)$ is the value quoted in the table (or L/M when $M > 1$).

At a confidence level C , the maximum likelihood function $L = L(S_m) \geq M/(1 - C)$ is used to define a source "discovery", and $L(S_u)/L(S_m) = (1 - C)/M$ is used to define the upper limit S_u . In this analysis, $C = 0.95$, which corresponds to demanding a 95% confidence level, or a maximum likelihood level of 20. Again, for ref. 14, the error in B could reduce L if the true value of B was in the range B to $B + E_B$. Where E_B is known, the tables show the L value corresponding to $B' = B + E_B$. Where the likelihood value exceeds 20 (confidence exceeds 95%), a source flux and errors are calculated in the manner described by Hearn; $L(S_{\pm}) = e^{-0.5}$. $L(S_m)$ is solved for S_+ and S_- .

In the testing of the data of ref. 15, $M = 960$ has been used; although the authors bunched their bins in $25 (5^\circ \times 5^\circ)$ groups, it appears that they maintained 960 values by using 1° steps. The work of Fichtel *et al.*¹⁶ may have $M > 1$, but unless $M \gg 20$, which is unlikely, the result is most probably not a statistical fluctuation. The remainder of Table 1 treats the data of the Case-Melbourne group¹⁷⁻²⁰. If one accepts the evidence for Sgr γ -1 in ref. 17, then the data of ref. 18 can be searched at, or near, the location of Sgr γ -1 for a source count—thus M is lowered. The rather large value of M used for G γ 2+3 and G γ 341+1 is deduced from the use of a basic bin size of $1^\circ \times 0.02$ over a region $220^\circ < \alpha < 310^\circ$ and $-59^\circ < \delta < -6^\circ$ (where the 0.02 is in units of $\sin \delta$). For the sources Cen γ -1 and G γ 327+2 reported at IAU Symposium No. 55 (ref. 20), the value of M for the number of bins searched is purely my estimate.

Shown in the last column of the table is the Poisson probability P of getting a number N or greater in M observations when the background is $B + E_B$ (or $E_B = 0$ if unknown). If p is the probability in one observation, $P = 1 - (1 - p)^M$ is used for M observations or attempts. It can be seen that examining the probability that the effect is a chance occurrence is not the same as testing for the probability or likelihood that the effect is due to a source. One expects 5% of independent tests of data to produce chance effects at the 95% confidence level, but considerably fewer will produce a source likelihood with 95% confidence. In general, source confidence levels obtained by the relative likelihood method in the range 95% to 99.9% will be a factor of 5 to 10 times lower than the corresponding simple Poisson probability "confidence level".

Cygnus-Cassiopeia Region

The data in this and the following groups have been analysed in the manner described for the previous group. Table 2 shows data for some of the results for this region. On the basis of the figures quoted, all the likelihood values are too small to be significant. The apparent source detection by Stepanian *et al.*⁷ in the region of Cassiopeia can be examined with normal statistics ($B = 1,770 \pm 9.2$, $N = 1,940$). This shows a 4.0 σ peak in 1 of 21 bins on 3 of 6 nights of observations; thus, $M \sim 126$ and $p \sim 2 \times 10^{-5}$, giving $P \sim 3 \times 10^{-3}$. If this value of $M = 126$ includes all degrees of selection, this region should be examined again for confirmation.

Table 3 shows data for the anticentre region. Here, again, there may be no non-statistical effects. The observation on CP 1133 (ref. 27) has not been confirmed by further measurements^{5,22,23} and is usually disregarded.

Pulsar Periodicity Searches

Analysis of a phase histogram for pulsar periodicity can be treated in the same way as a sky survey. Either the phase of the pulsar is known exactly and a particular bin can be designated (with $M = 1$), or all bins have to be searched. When M bins are regrouped in order to maximize an effect, the number of ways of regrouping multiplied by the number of bins present is still equal to M . The only exception to this is if, in a single purely objective manner, the regrouping is done before any search is made. For the case of the Crab Nebula pulsar, one has to make an objective decision as to whether an effect is sought at the main-peak position, the interpulse position, or both. In the data shown in Table 4, it is assumed that this decision has already been made on some physical basis; if

Table 1 Galactic Centre Region

Reference	Source	Background counts B	Error in B E_B	Observed counts N	Attempts M	Relative likelihood L	Upper limit (counts)	Flux (counts)	Poisson probability P (%)
Bennett <i>et al.</i> ¹⁴	Sgr XR-2	2×10^8 eV	34.1	—	48	≥ 1	32.9	$5.5^{+3.2}_{-2.6}$	1.4
		5×10^8 eV	10.0	—	17	≥ 1	19.2		2.7
		1×10^9 eV	2.5	—	8	≥ 1	—		0.42
		2×10^9 eV	1.0	—	2	≥ 1	6.7		26.0
Browning <i>et al.</i> ¹⁵	Source No. 1	16	± 1.3	29	960	~ 0.03	45.2	$64^{+16.9}_{-16.3}$	99.76
		33	± 2.6	50	960	~ 0.01	57.9		100.0
		22	± 1.6	35	960	~ 0.01	47.8		100.0
		8	± 0.6	19	960	~ 0.11	38.0		100.0
Fichtel <i>et al.</i> ¹⁶	Line source	210	—	274	≥ 1	$\sim 7.3 \times 10^3$	—	$64^{+16.9}_{-16.3}$	1.4×10^{-3}
Frye <i>et al.</i> ¹⁷	Sgr γ -1	Flights I+II	± 1.4	111	200	≥ 55.9	—	$40.9^{+10.9}_{-10.2}$	0.18
Frye <i>et al.</i> ¹⁸	G γ 2+3	All data	72.6	110	$\sim 3,330$	~ 1.22	94.5	$34^{+10.5}_{-9.8}$	8.29
	G γ 341	All data	68.0	101	$\sim 3,330$	~ 0.32	88.0		30.7
	Sgr γ -1	All data	68.0	102	~ 10	~ 157	—		0.07
Frye <i>et al.</i> ¹⁹	Lib β -1	All data	75.0	121	100	$\sim 1.4 \times 10^3$	—	$46^{+11.4}_{-10.7}$	6.3×10^{-3}
Frye <i>et al.</i> ²⁰	Cen γ -1	All data	41.0	68	(~ 100)	(16.4)	(64.4)		(0.71)
	G γ 327+2	All data	47.6	91	(~ 100)	(1.5)	(61.5)		(0.91)

any (rather than one) of these three possible effects is to be accepted, M should be increased by 3. The tests are as in the previous tables. In all these results, a measure of E_B is easy to obtain from the phase histogram. All the L values are therefore lower limits. The result from ref. 24 would have an L value greater than 20 if the true background was

$\lesssim B$. The final entry shows what a lower energy result²⁵ looks like when examined in the same way (despite its large M value).

Confidence in the Source Detections

Given the observational parameters B , E_B , N and M as described, it is possible, in general, to provide a reliable and

Table 2 Cygnus–Cassiopeia Region

Reference	Source	Background counts B	Error in B E_B	Observed counts N	Attempts M	Relative likelihood L	Upper limit (counts)	Poisson probability P (%)
Browning <i>et al.</i> ⁶	Source No. 8	20	± 2.0	39	576	≥ 0.36	52.6	31.63
		7	± 0.7	19	576	≥ 0.61	37.5	21.49
Niel <i>et al.</i> ⁹	Apparent No. 1	11.6	—	22	145	~ 0.27	34.8	44.88
		2	—	5	145	~ 0.06	18.2	98.59
		3	—	24	145	~ 0.06	34.1	95.39
		4	—	23	145	~ 0.06	33.5	95.39
		5	—	7	145	~ 0.06	20.6	97.05
		6	—	17	145	~ 0.04	28.6	99.82
		7	—	17	123	~ 0.16	30.1	66.69
		8	—	23	123	~ 0.06	32.8	96.55
		9	—	20	123	~ 0.11	31.8	80.00
		10	—	7	123	~ 0.04	19.9	99.95
Niel <i>et al.</i> ²⁶	Cygnus	165–15	—	81	123	~ 0.71	65.0	17.88
		283	—	325	(~ 10)	(~ 2.0)	(104)	(7.53)

Table 3 Galactic Anticentre Region

Reference	Source	Background counts B	Error in B E_B	Observed counts N	Attempts M	Relative likelihood L	Upper limit (counts)	Flux (counts)	Poisson probability P (%)
Forichon <i>et al.</i> ²¹	66–8	23.1	± 2.3	34	144	≥ 0.03	37.5		99.99
	65–43	13.5	± 1.3	27	144	≥ 0.39	38.6		33.22
	70–43	16.4	± 1.6	32	144	≥ 0.56	42.2		22.85
	76–41	27.5	± 2.0	47	144	≥ 0.56	50.4		22.85
	82–27	85.5	± 3.0	103	144	≥ 0.02	60.5		100.00
	83–22	51.5	± 2.0	66	144	≥ 0.03	52.5		99.97
	96–29	56.4	± 2.0	81	144	≥ 0.34	64.0		35.12
	Per γ -1	56.6	—	91	(100)	(67.1)	(34.4)		(46.31)
Frye <i>et al.</i> ²⁰	CP 1133	81.0	± 4.5	122	~ 4	≥ 241	—	$41 + 11.4$ $- 10.8$	0.05

Table 4 Pulsar Periodicity Searches

Reference	Source/energy	Background counts B	Error in B E_B	Observed counts N	Attempts M	Relative likelihood L	Upper limit (counts)	Flux (counts)	Poisson probability P (%)
Frye <i>et al.</i> ²⁸	NP 0532								
	Main $\lesssim 1.0 \times 10^7$ eV	27.0	± 1.3	47	3	> 57	—	20	$+7.2$ -6.6
	Main + Inter	54.0	± 2.6	75	3	> 5.0	48.6		3.26
Share <i>et al.</i> ²⁴	Main $> 1.0 \times 10^7$ eV	14.4	± 1.0	31	3	> 147	—	16.6	$+6.6$ -5.3
	Main + Inter	28.8	± 2.0	45	3	> 5.8	38.2		2.88
	Main $> 1.5 \times 10^7$ eV	32.0	± 1.4	48	1	> 16.6	33.6		1.0
Parlier <i>et al.</i> ²⁹	Main + Inter	64.0	± 2.8	82	1	> 5.0	39.4		3.9
	Main $> 2.0 \times 10^7$ eV	594	± 7.0	640	1	> 3.5	103		5.9
	Main $> 5 \times 10^7$ eV	153	± 2.2	180	33	> 0.20	77.6		60.8
Browning <i>et al.</i> ³⁰	Main + Inter	306	± 4.4	360	33	> 1.3	122		67.0
	Main $> 5 \times 10^7$ eV	8.4	± 0.7	17	5	> 3.1	23.7		5.9
	Main + Inter	16.8	± 1.4	29	5	> 3.0	30.3		5.9
Vasseur <i>et al.</i> ⁴	Main $> 5 \times 10^7$ eV	252	± 4.6	273	1	> 1.7	58.9		16
	Main + Inter	504	± 9.2	541	1	> 2.1	86.7		12
	Main $> 6 \times 10^7$ eV	2.73	$\pm 0.94^*$	3	1	—	6.7		71
Grindlay <i>et al.</i> ³¹	Main + Inter	8.19	± 2.8	13	1	> 2.2	15.7		13
	Main $> 6 \times 10^{11}$ eV	8.9	$\pm 1.4^*$	13	1	> 1.39	15.0		24
	Main + Inter	26.7	± 4.2	46	1	> 24.6		19.3	$+7.2$ -6.5
Porter <i>et al.</i> ³³	Main $> 2 \times 10^{12}$ eV	74	± 1.7	108	100	> 4.36	8.0		2.76
	Main + Inter	148	± 3.4	204	100	> 37.6	—	56	$+14.7$ -14.0
									0.28
Chatterjee <i>et al.</i> ⁵	CP 0950								
Harnden <i>et al.</i> ²⁵	Vela								
	$> 1 \times 10^{13}$ eV	30	± 1.0	59	5,000	> 4.3	73.9		2.47
	$> 2 \times 10^4$ eV	422	± 2.2	550	2,700	$> 9.3 \times 10^3$	—	128	± 23.8
									8.1×10^{-4}

* I have determined E_B from the variance of the data.

consistent measure of confidence in possible source detections in gamma ray astronomy. Once the use of Poissonian statistics can be justified and a good estimate of background count is made, the relative likelihood method can be used to calculate a source likelihood value L . If this exceeds a reasonable value (for example, $L=20$ for a 95% confidence level), a flux estimate and errors can be calculated; if not, only upper limits can be deduced. It should be noted, as mentioned earlier, that any confidence level C obtained from the L value [$C=100(1-1/L)\%$] is lower than the corresponding simple Poissonian "confidence level"; thus, the upper limit obtained by using the relative likelihood function may be larger than many of those already obtained and published.

From the L values obtained here it can be seen that in the Cygnus-Cassiopeia region (Table 2) there is little evidence for gamma ray sources, except perhaps at 10^{12} eV (ref. 7). Even the Poisson probability levels are not impressive. The evidence in the anticentre region (Table 3) is also rather weak. It may take some further time and observations before sources in regions other than at the Galactic centre and the Crab Nebula are accepted by all because of their convincing statistical evidence.

The need for a consistent and reliable statistical test has already been acknowledged; here I have tried to show how statistical tests on results in gamma ray astronomy can show which results are most reasonably ascribed to gamma ray sources. Less significant results can still be exciting and certainly deserve more investigation. If later investigation fails to show an effect, source variability should not be quoted in explanation unless the evidence for the presence of a source is convincing in the first case. It is for such cases in particular that a good estimate of the statistical probabilities or likelihoods involved is needed. The most appropriate test would appear to be the relative-likelihood test described by Hearn¹⁰.

I thank D. R. Hearn and G. G. Fazio for helpful discussions and suggestions.

Received October 16, 1972.

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The Search for Signals from Extraterrestrial Civilizations

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Although the technology exists for exchanging radio messages with extraterrestrial civilizations, a successful search for such civilizations among the many stars that might support them could take more than a thousand years, even if most habitable planets are occupied by communicative civilizations.

THAT the technology exists for sending and receiving radio messages over interstellar distances is not in doubt¹⁻⁵ so that, if there are similar technological civilizations based on stars not too distant from the Sun, we can, in principle, communicate with them. A problem, however, is to determine which star out of a large number of candidates is the home of a potentially communicative civilization⁶⁻⁸. The subject of this paper is the search problem of interstellar communication.

Occurrence of Habitable Planets

Even the most optimistic estimates of the frequency of occurrence of potentially communicative civilizations suggest

that a large number of stars will have to be searched before a civilization is encountered. If we let P_c be the probability that a given star has a communicative civilization, we may write

$$P_c = f P_{HP} \quad (1)$$

where P_{HP} is the probability that the star has a habitable planet in orbit around it, and f is the fraction of habitable planets with communicative civilizations. This fraction involves the probability that a communicative civilization will evolve on a habitable planet as well as the average lifetime of communicative civilizations^{6,9-11}.

It may be impossible to determine the value of f by other than empirical means, but P_{HP} is a quantity that can, in principle, be estimated from a knowledge of cosmogony and planetology. Dole¹², for example, has made a detailed estimate of the probabilities that planets on which Man could exist are in orbit about stars of different spectral classes. His considerations lead him to conclude that P_{HP} achieves a maximum value of 5.5% for stars of classes G0 to G4, and that 3.7% of stars in classes F2 to K1 have habitable planets; P_{HP} is zero for stars outside this range. Dole's estimates involve many assumptions, and improved values of P_{HP} will undoubtedly become available in time, but his values are adequate for present purposes. They show, first, that several tens of stars must be searched, even if f is of order unity and, second, that we can estimate P_{HP} for different stars and therefore can use this information to guide our search.

Strategy for the Search

One approach to the search problem is to assume that the other civilization will do most of the work, which implies that the search is limited to "supercivilizations" able to transmit detectable signals in all directions all the time^{5,13}. We could not do such a thing¹⁴, for the power requirement of an isotropic call signal detectable at a range of 100 light year is approximately equal to the world's present total power consumption^{2,15}. We can signal over interstellar distances only by using a large radio telescope to concentrate the radiated energy into a narrow beam. It would be possible to use a number of transmitters to send continuous signals to an equal number of target stars, but for such a strategy to have a reasonable chance of success, the number of transmitters would have to exceed

$$P_c^{-1} = (f P_{HP})^{-1}$$

This number is larger than 18, using Dole's values of P_{HP} , and it may be very much larger, since f may be small.

Even if there were several interstellar transmitters—and there are not—a strategy of continuous transmission to a select group of stars would not, however, be optimal. So as f is unknown, the probability of success for the transmitting civilization is proportional to the number of stars called regularly. In order to call the largest possible number of stars for a given level of effort, transmitter time must be shared among different target stars. How often, then, should a signal be sent to a given star?

Von Hoerner⁶ has analysed this problem in general terms, pointing out that it is necessary to develop an optimal search strategy for both transmitter and receiver, and then to assume that the target civilization will perform the same analysis and arrive at the same conclusion. I present here a strategy for which the probability of success can be evaluated, at least as a function of f .

An optimal search strategy should use all the information that we share with the target civilization. This includes the spectral classes of candidate stars and thus the values of P_{HP} ; it includes the optimal spectral region in which to work, the region where unavoidable background noise is minimal¹¹⁻⁵ and the distances to the candidate stars.

This last quantity provides the only indication we have as to how often we should look at any given star. The natural repetition period⁸ for a star at distance R is

$$T = \frac{2R}{c} \quad (2)$$

where c is the velocity of light. It is the time for a contact signal to travel to the star and for a reply to return. I shall assume that the transmitter sends a contact signal every T years to every star within range having a non-zero P_{HP} . Excess transmitter capacity would be used to increase the range of the search rather than to provide more frequent contact signals.

Although there are a number of ideas about the wavelength to use for contact signals^{1,5}, a search in frequency cannot be eliminated entirely because extremely narrow bandwidths must be used for interstellar communication². I shall not consider the frequency search explicitly, so for simplicity in the analysis I shall assign this task to the transmitter. Thus the contact signal to be sent out every T years will sweep slowly over the optimal spectral region, and the receiver may confine its search to a single frequency.

Probability of Success

With the transmitter strategy thus defined, it is possible to determine the optimal receiver strategy and evaluate the rate of success. From the point of view of the receiver, let us redefine P_c to be the probability that a given star has a civilization that sends a contact signal to the receiving star every T years at the wavelength on which the receiving civilization is listening. This redefinition introduces a corresponding change in the definition of the unknown fraction f , but no change in the known probability P_{HP} .

If, at the beginning of the search, the receiver devotes a period of time $\Delta\tau$ to listening to a given star, the probability that it will receive a call is

$$P_s = P_c \Delta\tau/T \quad (3)$$

The rate of success in the search is therefore $f P_{HP}/T \text{ yr}^{-1}$, where P_{HP}/T depends on the spectral class and the distance of the target star and f is the same for all stars. Because the repetition period T increases linearly with distance to the target star, the success rate is highest for the closest stars. Using Dole's figures¹⁷, we find a success rate for α Centauri of $1.3 \times 10^{-2} f \text{ yr}^{-1}$; for ϵ Eridani and τ Ceti the success rates are both about $1.5 \times 10^{-3} f \text{ yr}^{-1}$. For a G0 star at 100 light year, however, the success rate is $2.7 \times 10^{-4} f \text{ yr}^{-1}$.

But the receiver should not devote all its time to the closest star. This star may not be the home of a communicative civilization. After the receiver has devoted a large number n of randomly spaced listening periods $\Delta\tau$ to a given star, the probability that the receiver will have failed to receive a call, assuming that there is a transmitter associated with that star, is $\exp(-n\Delta\tau/T)$. The probability of success on the next look at the star is therefore

$$P_s(\tau) = f P_{HP}(\Delta\tau/T) \exp(-\tau/T) \quad (4)$$

where $\tau = n\Delta\tau$.

The optimal receiver strategy is now clear. The success rate is maximized if each listening period $\Delta\tau$ is devoted to the star for which $(P_{HP}/T) \exp(-\tau/T)$ is greatest. As the search progresses, the number of stars included in the search increases steadily, for each star within range the value of τ increases, and the instantaneous success rate grows steadily smaller. Each try, however, adds the greatest possible amount to the cumulative probability of success. How much time, on average, must elapse before success is achieved?

Suppose that there are N_i stars of spectral class i per unit volume and let $P_{HP}(i)$ be the probability that each of these stars has a habitable planet. After a total time t has been devoted to the search, following the strategy outlined above, the cumulative probability of success is

$$P_s = \frac{4}{3} \epsilon f t \quad (5)$$

where ϵ is the instantaneous success rate at time t given by

Table 1 Search Strategies for Various Distributions of Civilizations

Fraction of habitable planets occupied by communicative civilizations f	Average separation of communicative civilizations (light year)	Duration of search T_0 (yr)
1	24	1.4×10^8
10^{-3}	240	1.4×10^7
10^{-6}	2,400	1.4×10^{11}

$$\varepsilon = \left[\frac{\pi c^3}{32t} \sum_i N_i P_{HP}^4(i) \right]^{\frac{1}{4}} \quad (6)$$

From Dole's Table 18, $\sum_i N_i P_{HP}^4(i) = 1.24 \times 10^{-9}$ per cubic light year, so

$$\varepsilon = 3.3 \times 10^{-3} t^{-\frac{1}{4}} \text{ yr}^{-1} \quad (7)$$

$$\text{and} \quad P_s = 4.4 \times 10^{-3} f t^{\frac{3}{4}} \quad (8)$$

where t is expressed in years.

On the average, contact will be achieved when $P_s = 1$ or after a search that has lasted

$$t_0 = 1,380 f^{-4/3} \text{ yr} \quad (9)$$

Values of t_0 corresponding to several assumed values of f are shown in Table 1. Also shown is the average distance that separates communicative civilizations for these values of f .

We see that even with optimistic assumptions concerning the frequency of occurrence of communicative civilizations, the time required for a successful search is long. Of course, t_0 is, strictly speaking, telescope time devoted to the search, not total elapsed time. The duration of the search is therefore inversely proportional to the number of receiving telescopes and could be shortened by a massive effort. Alternatively, it is possible that the transmitter strategy I have assumed is incorrect, and that more frequent calls would be optimal, say m calls every T years. In this case the duration of the search differs from the values in Table 1 by a factor of $1/m$, provided the receiving civilization knows the value of m .

The conclusion, therefore, is disappointing. If every habitable planet has a communicative civilization there might be 50 such civilizations within 100 light year of us¹⁸. We possess the technology to exchange messages with this multitude of other worlds, if only we can find them. Unless my assumed transmitter strategy is seriously in error, however, or unless habitable planets are substantially more abundant than Dole has concluded, the problem of finding the other worlds is overwhelming. These circumstances may limit us to a search for supercivilizations¹⁶.

This research has been supported, in part, by a NASA grant.

Received January 5; revised September 11, 1972.

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Correlation between Cultural Characters and Pathogenicity in *Ceratocystis ulmi* from Britain, Europe and America

J. N. GIBBS & C. M. BRASIER

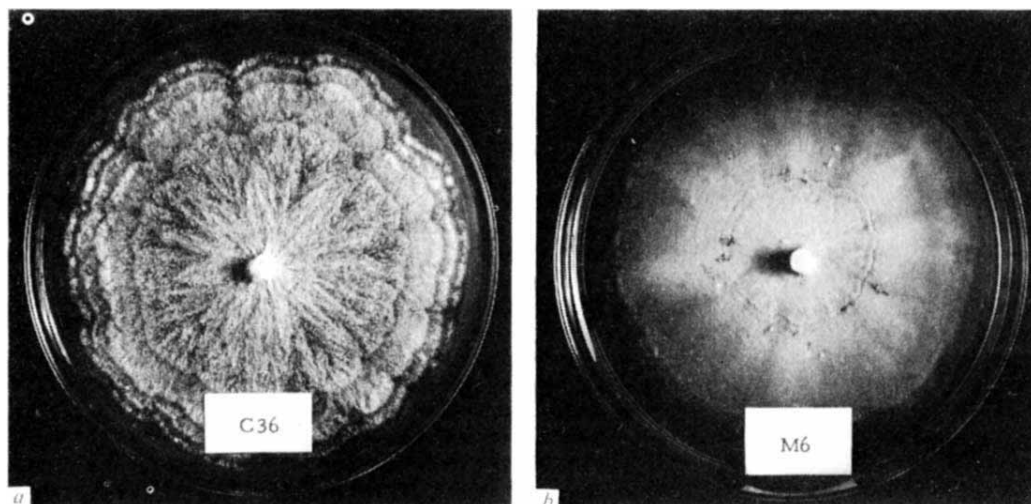
Forestry Commission Research Station, Alice Holt Lodge, Farnham, Surrey

Isolates of *C. ulmi* from Britain fall into two groups on the basis of cultural characters. Pathogenicity experiments show one group to be aggressive and the other to be non-aggressive. Some North American isolates resemble the aggressive isolates, while isolates from Northern Europe are non-aggressive.

THE epidemic of Dutch elm disease is continuing to develop in Southern England. A Forestry Commission survey was carried out in September 1971 and it was estimated that 4% of the elm population was dying or dead¹. By September 1972 the losses had doubled.

There is evidence for the existence of an aggressive strain of *Ceratocystis ulmi* in the worst affected areas². As a preliminary to further investigation of this strain the isolates used in the 1971 inoculation experiment at Alice Holt were examined for variation in culture. Differences between aggressive and non-aggressive isolates were apparent. This phenomenon was examined further by collecting isolates from a variety of

Fig. 1 Cultures of *C. ulmi* on 2% malt agar after 1 week in darkness at 18° C followed by exposure to light. *a*, Typical fluffy isolate from Chichester (Sussex); *b*, waxy isolate from Moreton (Oxfordshire).



disease situations. These included (*a*) the three main outbreak areas as identified in the 1971 survey; (*b*) recent outbreak areas, where a new centre of the disease was in evidence; and (*c*) non-outbreak areas where the disease was at its endemic level. Mass isolates were made from separate trees and single spore cultures derived; approximately 30 isolates were obtained from each locality.

Two Strains of *C. ulmi*

When radial growth rate experiments were run on 2% 'Oxoid Malt Agar' a relationship between growth rate and cultural morphology was established, and two groups of isolates could be distinguished. In one group the isolates were fast growing, with aerial mycelium aggregated into radial strands giving the culture a fibrous striate appearance. Clear zone lines were formed in response to diurnal fluctuation in light. These isolates were termed "fluffy" (Fig. 1*a*). Isolates of the second group were slow growing, the majority with a waxy yeast-like appearance, sparse aerial mycelium and little zonation (Fig. 1*b*). Fluffy isolates were remarkably uniform in cultural morphology, whereas the waxy isolates were more variable, both in degree of waxiness and in amount of aerial mycelium. There was virtually no overlap between the two groups on the basis of growth rate, and the great majority of isolates could be typed on this character alone.

Direct examination of the connexion between cultural characters and pathogenicity was made in comparative growth rate and inoculation experiments with 50 isolates from the

samples. These were selected isolates from established outbreak, recent outbreak and non-outbreak areas together with the original five aggressive and five non-aggressive isolates². Radial growth rates on 2% 'Oxoid Malt Agar' at 18° C were measured after five days in a replicated experiment. In pathogenicity tests, four-year-old clonal rooted cuttings of *Ulmus procera* (mean height 1.6 m) were inoculated in June 1972. The isolates were applied in randomized blocks with five replicates. Using the same inoculation technique as in 1971 (ref. 2), each tree received 5×10^5 spores. Disease was recorded in terms of percentage defoliation after two and four weeks, and as percentage stem dieback after eight and fourteen weeks.

The growth rate distribution of the isolates given in Fig. 2*a* shows the complete separation of the two groups. Fig. 2*b* shows the relationship between growth rate and pathogenicity; the complete separation of the two groups is again evident. The striking difference between the pathogenic behaviour of the groups is shown in Table 1. Defoliation proceeded rapidly with the fluffy type, under 5% of the leaves remaining after four weeks. By eight weeks, nearly 90% stem dieback had occurred, and after fourteen weeks only four of the 150 trees inoculated remained alive. In contrast, defoliation never exceeded 30% with the waxy type, dieback was slight, and inoculated trees frequently produced recovery shoots on the affected branches.

Table 1 Mean Growth Rate and Pathogenicity of Fluffy and Waxy Groups of *C. ulmi*

Item	Fluffy (<i>n</i> =30)	Waxy (<i>n</i> =20)
Growth rate mm day ⁻¹	3.94 (±0.04)	2.50 (±0.16)
Pathogenicity		
% Defoliation		
At 2 weeks	46.7 (±1.37)	24.2 (±1.37)
At 4 weeks	95.9 (±0.57)	30.1 (±1.37)
% Stem dieback		
At 8 weeks	88.3 (±1.79)	12.9 (±0.90)
At 14 weeks	99.4 (±0.40)	13.7 (±0.91)

n, No. of isolates. $P < 0.001$ for all differences between means of fluffy and waxy groups.

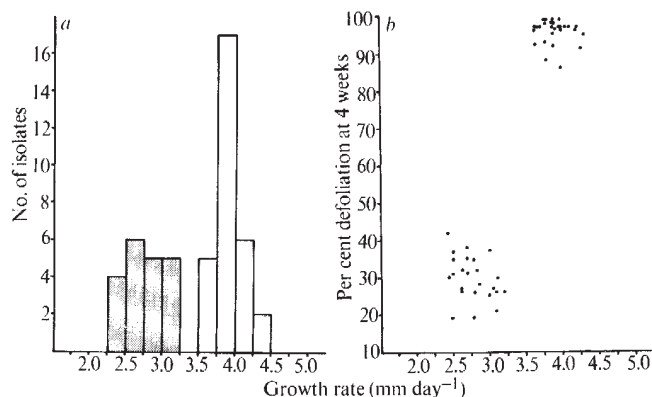


Fig. 2 *a*, Distribution of growth rates of fifty *C. ulmi* isolates showing the separation of fluffy and waxy groups. Tinted columns, waxy isolates; white columns, fluffy isolates. *b*, Relationship between growth rate and pathogenicity of the isolates.

The correlation between cultural characters and pathogenicity has enabled us to make a preliminary assessment of the relative frequency of the two strains, aggressive and non-aggressive, in different disease situations (Table 2). Of more than 350 cultures examined in 1972, only three have been found which could not be confidently assigned to one of the two groups.

Results showed that both strains were present in the outbreak situation with the aggressive strain predominant; in the non-

Table 2 Frequency of Aggressive and Non-aggressive Strains of *C. ulmi* in Different Disease Situations

Disease situation	No. of isolates	
	Aggressive	Non-aggressive
(a) Main outbreak		
Chichester (Sussex)	30	9
Orsett (Essex)	35	1
Tewkesbury (Gloucestershire)	28	8
(b) Recent outbreak		
Frensham (Surrey)	30	6
Ilchester (Somerset)	17	5
(c) Non-outbreak		
Moreton (Oxfordshire)	0	36
Swindon (Wiltshire)*	0	5
Botley (Oxfordshire)*	0	4

* Sampling limited by availability of material.

outbreak areas only the non-aggressive strain was found. The Moreton situation is particularly interesting as it represents a "flare-up" of the strain. A number of trees died in 1970 but no further deaths have occurred.

C. ulmi in Europe and North America

Evidence for the non-aggressive nature of four isolates from the Netherlands has been presented². Examination of these isolates has shown them to be of the waxy type, and a further eleven isolates, six from the Netherlands and five from northern France, were similar. It seems that, at least in parts of north-west Europe, the non-aggressive strain is the predominant if not the only strain to be found.

Of nine cultures from different parts of the United States, seven resembled the aggressive strain in growth rate and morphology, and two the non-aggressive. In an inoculation experiment, three of the former were compared with three aggressive and three non-aggressive British isolates on four-year-old trees of Commelin elm, of a clone of *Ulmus wallichiana* (301) and of seedling *U. americana*. Disease was recorded in terms of defoliation and stem dieback (Table 3).

The three American isolates were similar in pathogenicity to the British aggressive isolates in all elm varieties. The results of this limited comparison indicate that a strain of *C. ulmi* with similar characteristics to the British aggressive strain exists in the United States. The difference in pathogenicity between British aggressive and non-aggressive isolates was pronounced on Commelin and *U. wallichiana* but not on *U. americana*, which was highly susceptible to the non-aggressive isolates. This interaction demonstrates that Commelin and *U. walli-*

Table 3 Pathogenicity of North American and British Aggressive and Non-aggressive Strains of *C. ulmi*

Origin of isolates	Percentage defoliation after 4 weeks			Percentage stem dieback after 14 weeks		
	Com-melin (n=4)	U. wallich-iana (n=3)	U. ameri-cana (n=2)	Com-melin (n=4)	U. wallich-iana (n=3)	U. ameri-cana (n=2)
N. America						
(1) Athol (Mass.)	44	29	45	65	51	90
(2) Newhaven (Conn.)	24	32	65	28	48	90
(3) Kenilworth (Ill.)	50	40	57	72	63	92
Britain, aggressive						
(4) Essex	36	40	50	60	51	80
(5) Gloucestershire	36	33	63	74	47	96
(6) Sussex	55	45	52	59	68	93
Britain, non-aggressive						
(7) Cambridge	0	6	45	3	0	58
(8) Oxfordshire	0	2	67	0	0	82
(9) Denbighshire	0	4	37	0	0	54

n, No. of replicates.

chiana have a form of resistance which is not possessed by *U. americana*, perhaps a major gene. On this supposition the aggressive strain would possess the virulence gene to overcome this resistance and the non-aggressive would not.

Implications for the Dutch Elm Disease Problem

These results have considerable implications. First, recognition of the aggressive strains in culture has significance for the current disease situation in Britain, where an explosive epidemic is developing within a host population previously in equilibrium with the disease. It enables a ready distinction to be made between primary foci of the aggressive strain and local "flare-ups" of the non-aggressive strain, and it allows the aggressive strain to be detected in advance of the main disease front.

Second, the discovery of a correlation between cultural characteristics and pathogenicity is important for future research on the physiological basis of pathogenicity. Whether a faster growth rate can in itself account for the enhanced pathogenicity of the aggressive strain should be studied. Other morphological and physiological characteristics may well be found to be important.

The correlation also provides an opportunity for a study of genetic aspects of the disease. It is hoped that the survey of the compatibility types of British isolates now in progress will allow analysis of the genetic control of cultural variation and in particular the control and origin of aggressiveness. The relatively uniform appearance and behaviour of the aggressive isolates suggest that these are highly isogenic, possibly with a recent origin from a single clone. In contrast the waxy isolates may represent a sample of variation from the endemic gene pool.

Finally, some evidence has been presented that in the Netherlands and adjacent areas of north-west Europe a waxy non-aggressive strain appears to be predominant, whereas in the United States isolates of *C. ulmi* resembling the aggressive strain can be found. These observations are historically significant. Dutch elm disease was first described in Western Europe in 1918, and in Britain disease severity reached a peak between 1931 and 1937, and then declined. A high proportion of the elm population survived³. In Holland the disease was more destructive⁴, but in recent years there has been little infection among remaining elms. The disease spread from Europe to the United States about 1930, resulting in continuous heavy losses, and it has progressed across the continent. A possible explanation is that an aggressive strain was originally prevalent in all three countries. Supporting evidence is provided by contemporary descriptions of cultures with "fluffy type" morphology in the United States⁵, and in Europe^{6,7}. After this time for some reason the aggressive strain declined within the *C. ulmi* population of Britain and Europe, but not in the United States. We may now be witnessing its reappearance in Britain.

Whatever the exact course of events, it would appear that isolates of *C. ulmi* available in Europe in recent years for screening elm material for resistance have not provided a sufficiently stringent test. This underlines the necessity of incorporating as much variation in pathogenicity of the fungus as possible in programmes of breeding for resistant elms.

We thank Dr Hans Heybroek for diseased elm material from Europe, and Dr E. B. Smalley for cultures of American isolates.

Received November 28, 1972.

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On the Origin of Deuterium

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The origin of deuterium has always been a problem for theories of stellar nucleosynthesis. A general solution is proposed and shown to be applicable under several astrophysical circumstances in the light of new observations of the Galactic abundance of deuterium.

Cesarsky, Moffet and Pasachoff¹ have recently observed an absorption feature in the spectrum of radiation from the Galactic centre at 327.38837 ± 0.00001 MHz. They interpret this as arising from the ground-state hyperfine transition in deuterium near 91.6 cm which is analogous to the well-known 21 cm line of ordinary hydrogen. If they assume the feature observed to be due to noise they are able to set an upper limit

$$D/H < 5 \times 10^{-4}$$

while an analysis assuming the feature to be due to deuterium yields

$$3 \times 10^{-5} < D/H < 5 \times 10^{-4} \quad (1)$$

These results are to be compared with the terrestrial value of 1.5×10^{-4} and to the upper limit for the proto-solar value of 3×10^{-5} (refs. 2, 3).

Jefferts, Penzias and Wilson⁴ report line emission from a cloud within the Orion Nebula at 144,828 MHz and attribute it to the $J=2$ to $J=1$ transition in DCN. In a separate investigation of the $J=1$ to $J=0$ transition at 72,414 MHz, the hyperfine components expected for DCN have also been found⁵, setting the identification beyond reasonable doubt. The cloud in question probably has dimensions of the order of a light year and a mass of order $10^2 M_{\odot}$, considerably less than the Orion Nebula itself. The H II region of Orion, which is the part seen optically, has mass $\sim 10^3 M_{\odot}$, whereas a larger scale molecular cloud, detected in the 2.6 mm radiation of the CO molecule, has been estimated by Solomon (private communication) to have mass $\sim 10^5 M_{\odot}$.

The emission from the $J=1$ to $J=0$ transition is approximately as strong as that in $^1\text{H}^{12}\text{C}^{15}\text{N}$ and $^1\text{H}^{13}\text{C}^{14}\text{N}$, so

$$D/H = 6 \times 10^{-3} \quad (2)$$

a remarkably high value. The ratio determined in this way applies to D and H in combination with CN, not to D and H in atomic form. Thus the D/H ratio in the interstellar gas could still be comparable with the terrestrial value, or with the value of 3×10^{-5} referred to above.

The position concerning deuterium has therefore changed from doubt concerning its widespread existence in the Galaxy to one in which it is a reasonable inference that D/H of order 10^{-4} occurs on a large scale, and the problem of the origin of deuterium now seems more urgent than it did before. Together with R. V. Wagoner⁶, we found some years ago

that significant quantities of D could arise in a low density Friedmann universe. Defining a parameter h from the relation (applicable after $e^{\pm}$ -pair annihilation in the universe)

$$\rho_b = h T_9^3 \text{ g cm}^{-3} \quad (3)$$

where ρ_b is the baryon mass density and T_9 is the radiation temperature measured in units of 10^9 K, we found D/H comparable with the terrestrial ratio of 1.5×10^{-4} when $h \approx 6 \times 10^{-6}$. Setting $T_9 = 2.7 \times 10^{-9}$ K for the present temperature leads to $\rho_b \approx 10^{-31} \text{ g ml}^{-1}$ for the present baryon density, which is in good agreement with estimates which have been made of the average density of matter in galaxies by Oort⁷ and Shapiro⁸.

It is an unexpected feature of such a primordial mode of synthesis that the well-known cosmological parameter q_0 turns out to be small, and close to zero⁶, instead of the value 0.5 required to "close" the universe. Many cosmological investigations, the formation of galaxies for example, are much more awkward in hyperbolic models ($q_0 \approx 0$) than they are for $q_0 \geq 0.5$. One might seek to evade the resulting difficulties by arguing that, unlike an idealized Friedmann model, the actual universe is inhomogeneous. It might be supposed that the initial state of the universe was very patchy, with h falling below 10^{-5} in some places. On the other hand, the 2.7 K radiation background is exceedingly uniform, both locally and from one part of the sky to other distant parts of the sky. This uniformity, while not forbidding sufficiently fine scale inhomogeneities, would seem to us to constitute a warning against this line of argument.

It is also important in the new circumstances of the problem to reconsider possible astrophysical modes of origin for deuterium. Should it turn out that D/H is locally variable, astrophysical processes would be preferred to primordial synthesis, but if the D/H ratio is found to be universal then primordial synthesis would be preferred.

Investigations going back to the middle nineteen fifties have repeatedly shown that D is best produced astrophysically under non-thermodynamic conditions. We imagine α particles projected at high speed into an ionized gas composed mainly of hydrogen. If the gas temperature has some moderate value, say 10^5 K for definiteness, nuclear reactions leading to D production can occur. D can be knocked out of an α particle by a spallation reaction, and neutrons knocked out of the ^4He can subsequently be captured by protons of the ambient gas. For example, α particles entering gas at speed $c/3$ have a kinetic energy relative to the gas of ~ 200 MeV, which is 50 MeV per nucleon, and at such a bombarding energy the total cross section for all the reactions leading to D production is about $5 \times 10^{-26} \text{ cm}^2$, that is, 50 mb according to Audouze *et al.*⁹. Although the stopping cross section due to Coulomb scattering is greater than this, it is clear that a significant fraction of the α particles will produce a D nucleus. Because the D is thus formed within a comparatively low temperature gas it is not subject to subsequent breakup, except in rare cases where it happens to be hit by a further incoming α particle.

There are many ways in which this general idea can be used; for example, it can be applied to cosmic rays entering a cloud of gas. The production of D (and ^3He) through the spallation of ^4He was discussed a decade ago¹⁰ in connexion with magnetic flares in the solar surface. The nuclear physics involved is largely independent of the acceleration mechanism. But processes involving cosmic rays are not capable of explaining large D concentrations of the kind that have now been reported. Cosmic rays are too wasteful of their energy in this respect. D production reaches the geometrical cross section and thus occurs most efficiently when the energy per nucleon is about 30 MeV. Energies as high as several GeV, which is where the main reservoir of cosmic ray energy lies, are not required. For greatest efficiency we must look therefore to processes involving speeds $\sim c/3$.

Such speeds have indeed been found for shock waves generated by stellar explosions¹¹. The shock wave starts in the region of the explosion at a speed not much different from the speed of sound, which is always much less than c . As it travels outwards into the lower density regions of the envelope, however, the wave speeds up. It is therefore in the outer envelope that speeds of the required order have been reported in previous investigations.

The shock wave condition we have in mind can be applied much more generally than to a supernova. The basic requirement is for a supply of radiant energy (or of relativistic particles) to emerge from some local source into a diffuse outer envelope of gas containing ^4He . The larger the energy supply the better. For a supernova we expect $\sim 10^{50}$ erg to be available, which is much less than a case reported recently by Appenzeller and Fricke¹²⁻¹⁴. Their work is concerned with objects having masses between 10^5 and $10^6 M_\odot$ —that is, masses of the order of the whole Orion Nebula. Under suitable conditions nuclear energy generation in a time scale of a few thousand seconds, yielding $\sim 10^{56}$ erg, can lead to expansion and disruption of the whole object. Because most of the energy is taken up in the radiation field, it is possible that a bubble of radiation, say with total energy $\sim 10^{55}$ erg, may work its way to the outer part of the object and may propagate thence into a surrounding diffuse cloud. Outbursts involving relativistic particles, also with energies of $\sim 10^{55}$ – 10^{56} erg, occur in radio galaxies. Indeed, outbursts from radio sources involving energies up to $\sim 10^{60}$ erg have been considered.

The energy in question, whether radiant or in the form of relativistic particles, would escape from the source at speed c if it were not for the material of the surrounding envelope. But as the envelope becomes more tenuous the radiation is able to push material ahead of it at speeds which approach more and more to c . In such circumstances the material of the envelope is accelerated forward impulsively at a shock front, and nuclei present in it are subject to spallation if the shock becomes violent enough. Deuterium is then formed, either by direct spallation of ^4He or from neutrons coming from ^4He , the neutrons being captured subsequently by protons downstream of the shock.

The properties of shock waves are usually investigated through the equations of continuum mechanics. The complex physical processes taking place at the shock front are idealized by a discontinuity, much as an impulse to a body is idealized in classical mechanics. We define v_i, v_s as velocity components normal to the shock, with subscripts i, s referring to conditions upstream and downstream of the front respectively. For simplicity, taking hydromagnetic effects to be small, and taking a frame of reference in which the front is stationary, we have the following conservation relations across the front:

$$\begin{aligned} \rho_i v_i &= \rho_s v_s \\ p_i + \rho_i v_i^2 &= p_s + \rho_s v_s^2 \\ u_i + \frac{p_i}{\rho_i} + \frac{1}{2} v_i^2 &= u_s + \frac{p_s}{\rho_s} + \frac{1}{2} v_s^2 \end{aligned} \quad (4)$$

where p is the pressure and u is the internal energy per unit mass. Terms in p_i, u_i are small in our case. Omitting them, we can satisfy equations (4) with

$$\rho_s = \rho_i \frac{\gamma + 1}{\gamma - 1}, \quad v_s = v_i \frac{\gamma - 1}{\gamma + 1}, \quad p_s = \frac{2\rho_i v_i^2}{\gamma + 1} \quad (5)$$

where

$$(\gamma - 1) \rho_s u_s = p_s \quad (6)$$

γ is the ratio of specific heats downstream of the shock, and is close to $4/3$ in a radiation dominated problem. Putting $\gamma = 4/3$ in equation (5) we get

$$\rho_s = 7\rho_i, \quad v_s = v_i/7, \quad p_s \simeq \frac{1}{3} a T_s^4 = \frac{6}{7} \rho_i v_i^2 \quad (7)$$

The third of these equations can be regarded as determining v_i when T_s and ρ_i are given. The value to be used for T_s depends on the energy supply, while ρ_i is the density of the envelope upstream of the shock. From here on, we shall be concerned with situations in which T_s is high enough in relation to ρ_i for the resulting value of v_i to be of order $c/3$.

Material flowing through the front experiences a change of velocity $v_i - v_s = 6v_i/7$, which for $v_i = c/3$ is $2c/7$. If we now think of the material in terms of individual particles, instead of from the point of view of continuum mechanics, the particles experience a change in velocity of $2c/7$ as they pass from being upstream to being downstream of the shock. We have to think of the front as possessing a finite depth and of particles from upstream experiencing collisions as they pass through a finite shock zone. As a velocity of $2c/7$ is equivalent to a bombarding energy of ~ 40 MeV per nucleon we have a situation similar to that discussed above for α particles projected into a stationary gas. Thus ^4He present in material upstream of the shock will be subject to fragmentation as it passes through the front.

The present situation is actually more favourable to fragmentation than the case of α particles projected into an ambient gas, because all particles in the shock zone have come from upstream and have therefore experienced collisions. About half of the bombarding energy will be transferred to electrons, but otherwise the bombarding energy will be retained as random motions of protons and α particles within the shock zone, and the α particles will therefore be subject to break-up over the whole of the time they are within the shock zone.

Material may be considered to have flowed downstream of the shock when electrons have had sufficient time to radiate the kinetic energy they have acquired from the heavy particles. Radiation through bremsstrahlung occurs with cross section

$$\sigma_{\text{brems}} \simeq 4 \frac{e^2}{\hbar c} \left(\frac{e^2}{mc^2} \right)^2 \ln(2E) \quad (8)$$

where E is the electron energy in units of the rest mass. In our case the electron energies are ~ 20 MeV, and the logarithmic term in equation (8) is about 4, so the cross section is ~ 10 mb. This is about five times less than the spallation cross section. On the other hand, an electron of energy 20 MeV has a velocity about five times greater than a proton of the same energy. Hence

$$\langle \sigma v \rangle_{\text{Bremsstrahlung}} \simeq \langle \sigma v \rangle_{\text{Spallation}} \quad (9)$$

from which it follows that an appreciable fraction of the α particles must be fragmented by the time they pass downstream of the shock.

At this stage we have to distinguish two cases according to whether the gas density is high enough for neutrons from spallation to be captured by protons, or not. The former case

is more efficient in its deuterium production by an order of magnitude. Considering this case, and taking the material of the initial cloud to have the usual helium concentration of ~ 0.25 by mass, and using say 40% for the fraction of the helium experiencing complete spallation, we arrive at a D concentration downstream of the shock of 0.1. Thus the mass density of D downstream of the shock is $0.1 \rho_s$. Using (7), and setting $v_1 = c/3$, we have

$$\begin{aligned} \rho(D) = 0.1 \rho_s &= \frac{7}{10} \rho_1 = \frac{7}{10} \times \frac{7}{18} \frac{a T_s^4}{v_1^2} \\ &= \frac{49}{20} \frac{a T_s^4}{c^2} = 2.7 \times 10^{-21} a T_s^4 \quad (10) \end{aligned}$$

or $a T_s^4 / \rho(D) = 3.7 \times 10^{20} \text{ erg g}^{-1}$

This is a relation between energy supply and deuterium production. The energy "cost" in the radiation field to produce one gram of deuterium is seen to be $\sim 3.7 \times 10^{20} \text{ erg}$. So to produce an average D/H ratio equal to the proto-solar value of $\sim 3 \times 10^{-5}$ throughout a mass M requires $\sim 10^{16} M \text{ erg}$, from which we see that to obtain D/H $\sim 3 \times 10^{-5}$ throughout the Galaxy requires $\sim 3 \times 10^{60} \text{ erg}$. Because this value is within the range that can be contemplated it seems possible for the present process to give a galactic deuterium concentration comparable to the initial solar system value.

The present considerations require neutrons from the spallation of ^4He to be captured by protons. For this condition to be satisfied ρ_1 must not be much less than $\sim 10^{-8} \text{ g cm}^{-3}$. Otherwise the efficiency of D production is reduced by an order of magnitude, and the energy requirement is increased correspondingly. In the rest of this article we shall take $\rho_1 = 10^{-7} \text{ g cm}^{-3}$ for definiteness, and will consider the rates which are then operative for various processes.

The first question to be asked is: what will be the order of the depth of the shock zone? Taking $\sim 3 \times 10^{-7} \text{ g cm}^{-3}$ as the average density within the shock zone, and noting that $\langle \sigma v \rangle_{\text{Bremsstrahlung}} \simeq 3 \times 10^{-16}$, we see that the electrons lose their energy in a time $\sim 2 \times 10^{-2} \text{ s}$. In this time material flowing at a mean speed of say $\sim v_1/2 = c/6$ travels a distance $\sim 10^8 \text{ cm}$. This gives the order of the depth of the zone.

To prevent radiation downstream of the shock from simply streaming through the front it is necessary that the optical depth of material within the shock zone shall be greater than unity. For the numerical values considered in the previous paragraph there are about 30 g of material per unit area of the front. This is sufficient to dam back the radiation through Thomson scattering by the electrons. Before the radiation can penetrate the shock zone new material is then added from upstream. This circumstance does not depend on the particular numerical values used here. It depends essentially on the Thomson scattering for radiation being much larger than the bremsstrahlung cross section, $\sim 10^{-24} \text{ cm}^2$ compared with $\sim 10^{-26} \text{ cm}^2$.

Once bremsstrahlung transfers energy from the electrons to the radiation field other processes become involved, particularly Compton scattering. Because most of the bremsstrahlung energy from 20 MeV electrons consists of γ rays above 1 MeV, Compton scattering has a complex behaviour, certain scattering angles augmenting the radiation field, others transferring energy back to the electrons. A refined calculation would be necessary in order to consider such effects in detail. Here we shall simply take relation (9) to represent the order of magnitude of the relation of radiation losses to spallation. The conclusion from (9) is that an appreciable fraction of α particles are fragmented by the time they pass downstream of the shock.

Deuterium produced within the shock zone will itself be subject to spallation, as will be ^3He and T. Consequently we shall not regard D production as being due to the immediate spallation of ^4He , but as arising from neutrons released

in the breakup of ^4He . The lifetime of the neutrons against weak decay ($\sim 10^3 \text{ s}$) is very long compared to the time spent ($\sim 10^{-2} \text{ s}$) in the shock zone. So the neutrons move downstream to regions where thermodynamic conditions can be considered to be established. Putting $\rho_1 = 10^{-7} \text{ g cm}^{-3}$, $v_1 = c/3$ in (7) gives $T_s \simeq 7.6 \times 10^6 \text{ K}$, which is far too low for deuterium formed by $n + p \rightarrow D + \gamma$ to be subject to spallation or to photodisintegration. The $\langle \sigma v \rangle$ value for this reaction is $7 \times 10^{-20} \text{ cm}^3 \text{ s}^{-1}$, so for a hydrogen density $\simeq 7 \times 10^{-7} \text{ g cm}^{-3}$ downstream of the shock it takes some 30 s for the neutrons to be captured by protons. Because this is much less than the neutron half-life, although amply long enough for the neutrons to flow downstream well clear of the shock, we conclude that essentially all neutrons go to form D.

To recapitulate to this point: Normal thermonuclear processes generate ^4He from hydrogen with very little production of deuterium. But if ^4He can be shaken loose into its constituent neutrons and protons in abnormal situations, such as those in the front of a high speed shock wave, conditions become favourable to D production provided the gas density is not so small that the neutrons decay before they are captured by protons. Conditions are then also favourable in that the temperature $T \simeq 7.6 \times 10^6 \text{ K}$ behind the front leads to a lifetime for $D(p, \gamma)^3\text{He}$ of $\sim 10^{10} \text{ s}$, which is much longer than the time required for a local exploding object to disperse. The D/H ratio can be locally higher than the terrestrial value by as much as 10^2 , but after averaging with other exterior material the D/H value will be lowered. If the estimate of 3×10^{-5} for the proto-solar value is typical of the interstellar medium as a whole, then the process described here could be the origin of all the deuterium in the Galaxy. Accompanying the production of D in the shock front will be that of a smaller amount of ^3He , perhaps enough to yield the proto-solar values ($\sim 10^{-5}$) taken by Black³ as typical. Further, because the explosion which produces the shock wave may well be due to nuclear energy generation converting a substantial fraction of hydrogen into helium, this process may also be the source of galactic ^4He . This brings us back (see pp. 23, 24 of ref. 6) full circle to the fact that conversion of hydrogen into helium in one part in four by mass yields the full energy of the background microwave radiation at 2.7 K and once again forces us to ask if there could have been a mechanism which provided the necessary thermalization. So far we have found no plausible affirmative answer to this question, but the coincidence of the numbers remains puzzling.

We thank Dr Klaus Fricke for discussions on the production of shock waves in the implosion-explosion of massive objects, and the authors of refs. 1, 4 and 5 for informing us of their work in advance of publication. This work was supported in part by the National Science Foundation.

Received December 12, 1972.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Fine Structure of the Jupiter Radio Bursts

SHORT pulses with a time scale of milliseconds and fractions of a millisecond have been observed in the radio emissions of Jupiter at wave frequencies near 20 MHz since 1956 (refs. 1 and 2). Little is known about these "S" bursts, however, although it has been established³ that their occurrence depends strongly on the orbital position of the satellite Io. Further, Gordon and Warwick⁴ were able to show from a small set of observations that the bursts may have a relatively narrow radio frequency bandwidth (< 50 kHz) and that their centre frequency may change rapidly with time at the rate $25\text{--}35\text{ MHz s}^{-1}$, at least near 26 MHz.

Ellis⁵ predicted that radio bursts with these general frequency-time characteristics may result from the passage of electron streams of about 10 keV energy through the spatially varying magnetosphere of Jupiter, although further work along these lines was handicapped by the inadequate detail of the available spectrograms. These had a time resolution of 10 ms and a frequency resolution of 50 kHz. An improvement in each of these parameters of at least an order of magnitude seemed to be essential to reveal the fine structure of the bursts.

In the observations reported here a time resolution of 0.5 ms combined with a frequency resolution of 10 kHz has been attained at radio frequencies using the technique of time-expansion spectrographic analysis previously used in the audio frequency range (ref. 6 and a private communication from R. L. Dowden). Signals received in the frequency range 15–18 MHz were reduced in frequency to 0–3 MHz and recorded on a standard helical scan video recorder. The video tape was subsequently replayed in the single frame replay mode at the

rate of one 20 ms frame every 8 s. A single channel sweep frequency spectrograph with a bandwidth of 10 kHz, a frequency range 0–3 MHz and a sweep time of 8 s analysed the frequency-time information in each frame. No internal modifications to the video recorder were necessary. The antenna which was used was a new general purpose radio telescope in the form of a filled aperture array, 2,000 foot square (ref. 7). It has a continuous frequency range of 2–20 MHz and operates as a transit instrument with four independent beams under computer control. The positioning of the beams takes 2 ms.

One beam was switched to the declination of Jupiter for ten minutes each day between May and September 1972. Video recordings were made of the signals, mostly between 15 and 20 MHz, while chart records were made of the signal intensity at six frequencies between 2 and 20 MHz. Several real time sweep-frequency spectrum analysers were used for low time resolution observations.

Video tape recordings of the rather rare S burst events were obtained during the transits of June 18 and September 13, 1972, some thousands of individual bursts being recorded. Figs. 1 and 2 show examples of different types of bursts observed on June 18. Most numerous were multiple quasi-periodic bursts with a frequency-time slope of 12.5 MHz and a frequency range of approximately 1 MHz (Fig. 1*a* and *b*) occurring at the rate of about 30 s^{-1} . Their bandwidth was in the range 10–50 kHz and single frequency duration 1–5 ms. The overall duration of each burst was about 60 ms. They probably correspond to the millisecond S bursts observed at fixed frequencies by Slee and Gent² and will be called S_1 bursts. Fig. 1*b* shows an example of a variant of the type S_1 in which a Z-shaped pattern was observed on the spectrograms.

A second class of burst is illustrated in Fig. 2*a*. These occurred at about ten times the rate of the S_1 bursts in a given time interval and showed both positive and negative frequency-

Fig. 1 *a*, Spectrograms of type S_1 Jupiter bursts recorded on June 18, 1972. Note the splitting of the frequency-time trace which can be seen in the extreme right-hand burst. The curvature of the traces towards the lower edge of each record is due to nonlinearity in the frequency sweep. *b*, Examples of a rare variant of the type S_1 burst in which the frequency-time trace shows a characteristic Z shape.

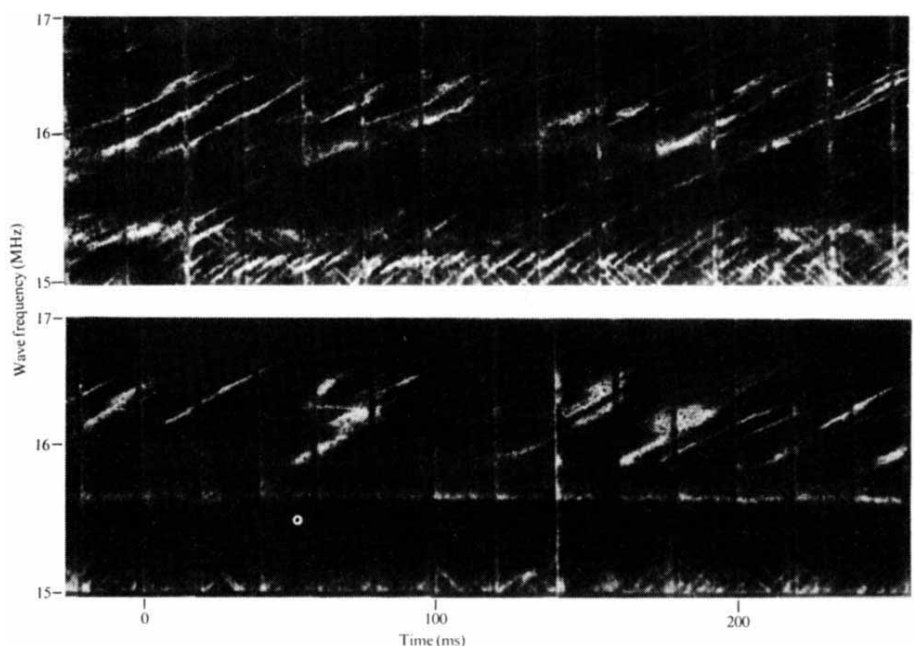
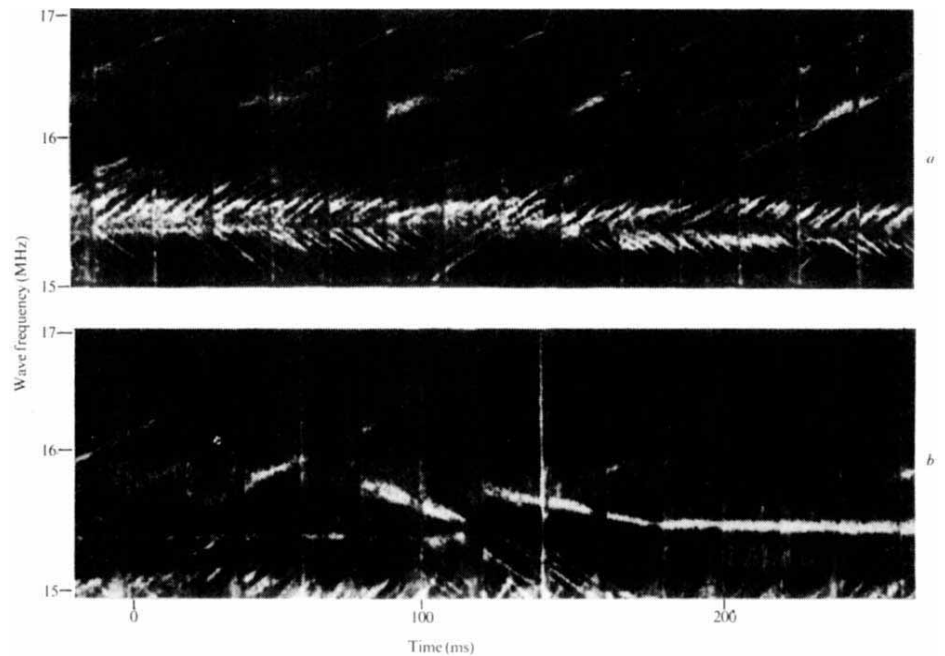


Fig. 2 *a*, Type S_2 herringbone bursts concurrent with type S_1 ; *b*, shows an example of a class of bursts of wider bandwidth and longer duration, typically 100 kHz and 0.1–1 s respectively.



time drifts of $\sim 10 \text{ MHz s}^{-1}$, sometimes simultaneously. These S_2 bursts were somewhat similar to the herringbone solar radio bursts, and with a single frequency duration of a millisecond or less may correspond to the sub-millisecond bursts previously observed².

On a time scale of 100 ms, a class of bursts with no particular frequency-time slope (Fig. 2*b*) was recorded, their bandwidth being typically 100 kHz and overall duration 0.1 to 1.0 s.

The S_1 and S_2 bursts are shown in more detail in Fig. 3*a* and *b*, in which the signal amplitude is displayed as a function of frequency and time for a time duration of 20 ms and a frequency range of 200 kHz. The crossing of the frequency-time traces which can be seen near the lower edge of Fig. 1*a* is shown clearly in Fig. 2*b*.

The observed frequency-time slope of the bursts of $10\text{--}12 \text{ MHz s}^{-1}$ at a wave frequency of 15 to 16 MHz is consistent

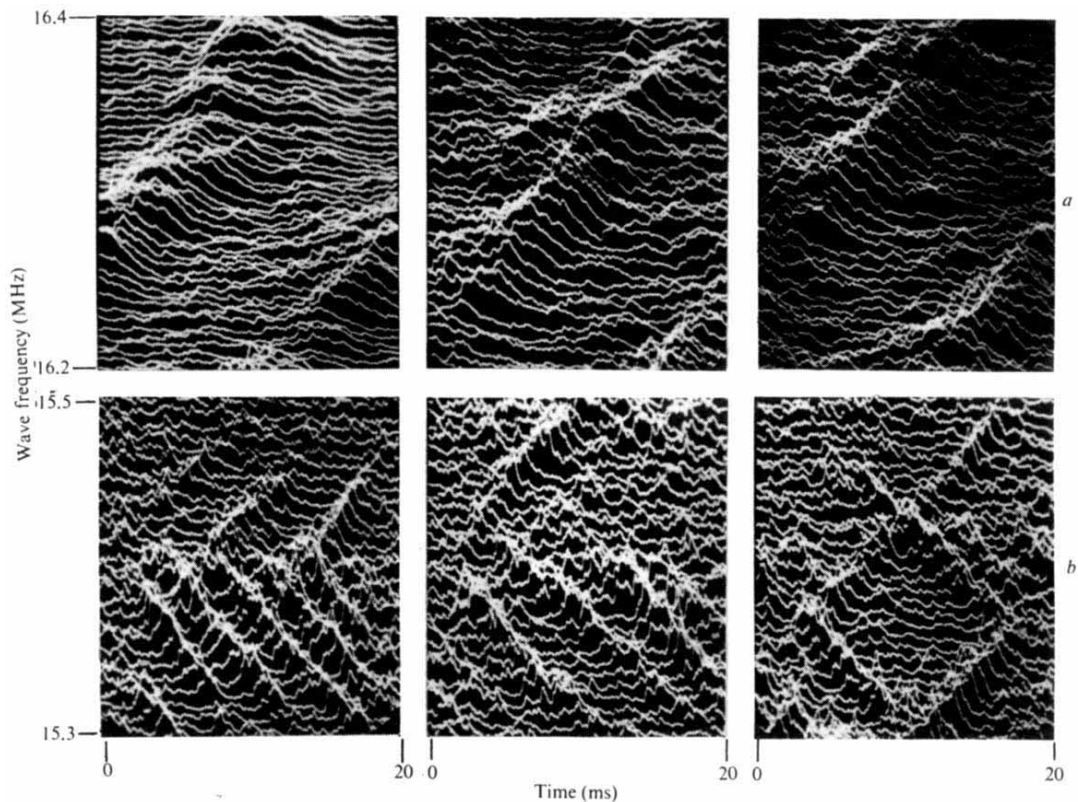


Fig. 3 Records showing examples of the variation of amplitude versus frequency and time of type S_1 (*a*) and type S_2 (*b*) bursts on an expanded frequency-time scale. The last section of *b* shows in more detail the crossing of the frequency-time traces which can be seen near the low frequency edge of Fig. 1*a*. Post-detection integration time constant 2.0 ms (*a*) and 0.5 ms (*b*). Frequency resolution of analyser 10 kHz. The separation of the traces in frequency is 5 kHz.

with the observations of Gordon and Warwick⁴ of 25–35 MHz s^{-1} near 26 MHz if it is assumed that electrons of energy 8–10 keV were responsible on both occasions. The change in the frequency-time slope between the two frequencies is just that which would be expected if the radiated wave frequency is near the local cyclotron frequency of electrons in the magnetosphere⁵ and if the rate of change of frequency with time arises from the longitudinal speed of the electrons and the spatial variation of the magnetic field.

As well as looking like solar radio bursts on the frequency-time plane, the herringbone bursts also bear a remarkable frequency-time resemblance to VLF emissions from the Earth's magnetosphere triggered by artificial electromagnetic wave transmissions from VLF ground stations⁸. It is believed in this latter situation that the incident electromagnetic radiation leads to coherent cyclotron radiation from a pre-existing electron stream in the vicinity of a region where the incident wave frequency, magnetic field intensity and electron energy are sufficient to satisfy the doppler-shifted cyclotron resonance condition⁸. The herringbone pattern would then arise from radiating electrons moving in both directions along a magnetic field line away from the resonance region.

It has been suggested^{5,9} that the orbital motion of Io relative to the Jupiter magnetosphere causes the generation of Čerenkov electromagnetic radiation at low radio frequencies (of the order of 10^5 Hz) where the phase speed of propagation is less than the relative orbital velocity of the satellite. Calculation has shown that such radiation would travel inwards towards the planet along ray paths which would bring it into the regions above the Jupiter ionosphere, thought to be the source of the Io-controlled decametric radiation. In this event cyclotron resonance between the inward travelling radiation, electron streams of keV energy, and the outward decametric radiation might be expected to lead to the observed frequency-time patterns of the bursts.

This investigation was supported financially by the Australian Research Grants Committee.

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Received October 31, 1972.

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Further Data on the Corsica–Sardinia Rotation

THE suggestion that the Corsica–Sardinia microplate rotated counter-clockwise with respect to France and Italy seems to be widely accepted today^{1,2}, but the dating of the rotation is still not satisfactorily settled. Alvarez³, reviewing all data on hand, proposed an age of 11.5 m.y. for the start of the motion, and 6 m.y. as the time when the motion ended.

We now propose a different approach to this problem, based on the radiometric age of metamorphism in the Apuane Alps (Northern Tuscany, Italy)⁴ and on the general tectonic picture of the Northern Apennines.

The Apuane Alps are located along the western (Tyrrhenian) coast of North–Central Italy, between the parallels of Pisa and La Spezia (Figs 1 and 2). Carrara is well known for its white

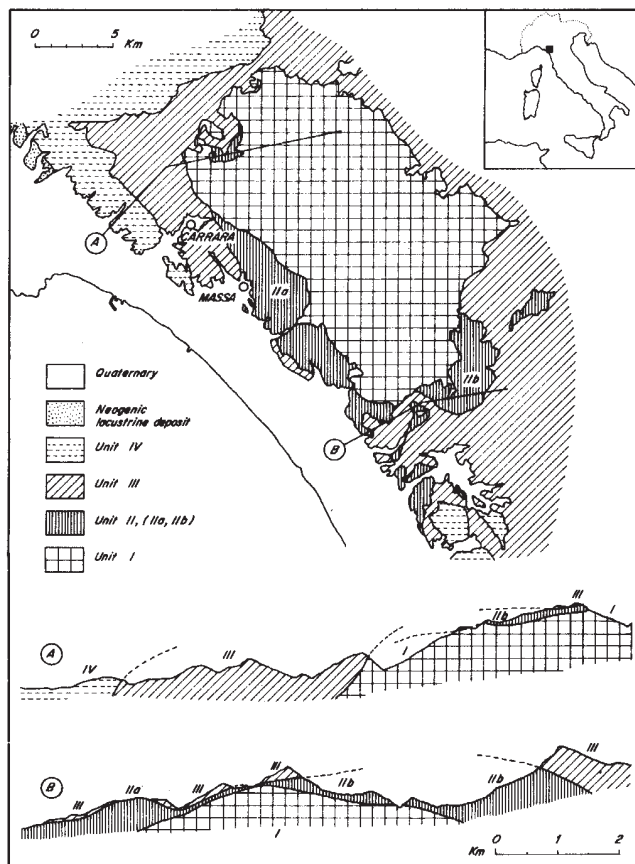


Fig. 1 Tectonic sketch-map and cross sections of the Apuane Alps. For comments see text.

marbles. From a geological point of view this mountain range, approximately 35 km long, is a tectonic window cut in the pile of nappes by which the northern Apennine is built up⁵.

Four tectonic units or groups of units are superimposed in the Apuane Alps. The lowest (Unit I), outcropping in the core of the massif, has at the base a phyllitic formation of undefined Palaeozoic age, then Permo-Triassic volcanics (of acidic type) and arkoses, Upper Triassic dolostones, Jurassic massive limestones, cherty limestones and radiolarites, then Cretaceous-Eocene marly shales, followed by turbiditic sandstones (possibly Oligo-Miocene).

Unit I is tectonically overlain by Unit II. The stratigraphy of Unit II is similar to that of Unit I, apart from small differences. The Triassic pre-carbonatic sequence is here represented by a thick group of detrital formations ("Verrucano") with calcareous lenses; Permian volcanics and their (partly) Triassic alteration products are lacking; also, evaporites are found associated with dolostones in the Upper Triassic deposits.

In Fig. 1, Unit II has been subdivided in two subunits (IIa and IIb) because its detrital base (subunit IIa) chiefly outcrops in the western side of the Apuane Alps, with a peculiar tectonic style, and its Mesozoic carbonatic rocks and Tertiary flysches are found only in the southern and southeastern parts of the massif (subunit IIb). Differences in metamorphism also justify this subdivision.

Unit III, the "Tuscan Nappe", is made up of a stratigraphical series ranging in age from Upper Triassic to Oligo-Miocene, in many aspects similar to the underlying units.

In turn Units I, II and III are covered by a complex pile of tectonic units, comprehensively called "Ligurids", which contain, among other rocks (such as the chaotic mélange known as "argille scagliose"), serpentines, gabbros and basalts.

Units I, II and III can be referred, broadly speaking, to a

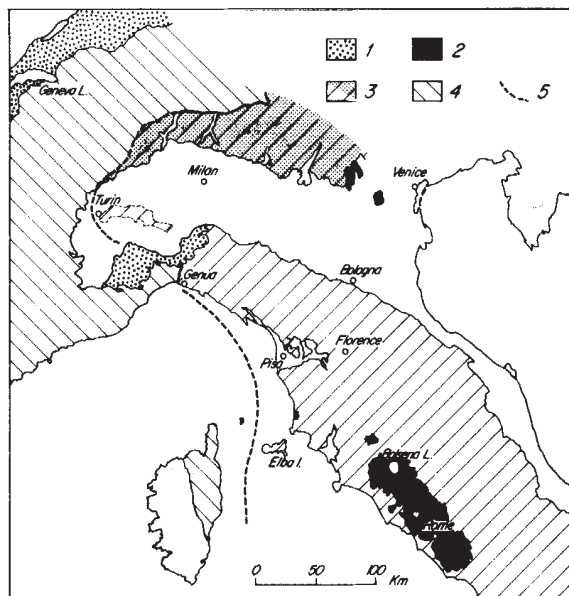


Fig. 2 Relations between Alps and Apennines north of the latitude of Rome. 1, Molassic peripheral basins. 2, Recent (tertiary and quaternary) volcanics. 3, Apenninic (not shaded) and Austro-Alpine Units (shaded). The outcrops of Apenninic rocks east of Turin (Monferrato hills) are partially covered by molassic deposits. 4, Alpine Units. Slabs of Apenninic Units involved in the tectonics of the Alps are not shown. 5, Separation line between Alps and Apennines.

continental shelf sedimentary basin, but the Ligurid complex indicates an oceanic environment.

Metamorphism affected the two lower units (I and II), contemporary with internal deformation; in Unit I it reaches the lower greenschist facies (paragenesis with quartz-albite-muscovite-chlorite). But subunit IIb shows the same metamorphic grade as Unit I, and subunit IIa underwent a stronger and different metamorphism (paragenesis with quartz-albite-muscovite-chlorite-biotite; presence of kyanite in some levels). These differences in metamorphism between IIa and IIb further justify the splitting of Unit II into two subunits. Unit III is not metamorphic.

The northern Apennine Range, of which the Apuane Alps are a part, is strictly connected to the Alps: these two ranges are undoubtedly paired from the latitude of the Corsica-Tuscany couple up to the Monferrato hills (which belong to the Apennines) between Genoa and Turin⁶ (Fig. 2). The western Alps-northern Apennines system is flexed into a horseshoe shape, thus representing an orocline in Carey's sense⁷.

The counter-clockwise rotation of the Corsica-Sardinia microplate is considered to be responsible for, or at least related to, the flexing in plan of the sedimentary basins out of which the northern Apennines originated.

In a pre-rotation reconstruction, Corsica and Sardinia were aligned in a NE-SW direction. The same was true for the sedimentary basins of northern Apennines and western Alps, which were limited to the NW by the European plate and to the SE by the Apenninic-Dinaric foreland. The middle of the trough, of oceanic (eugeosynclinal) nature, was occupied by the Ligurian and Piedmontese Units.

To the NW were the miogeosynclinal basins of western Alps (Briançonnais and Delphino-Elvetic), and to the SE those of the northern Apennines, in which Units I, II and III were forming. Most of the eugeosynclinal Ligurian Units were deformed during Eocene when they were thrust northwestwards on to the European block. Meanwhile sedimentation was still going on in the miogeosynclinal basins of the Apennines, where Units I, II and III were depositing.

At the beginning of the Miocene, after very thick turbiditic beds were deposited in these basins, compressional movements

began to affect Units I, II and III. Metamorphism interested Units I and II together with internal deformation which was therefore characterized by a high degree of plasticity. At roughly the same time, units of the Ligurian complex, already partly folded and thrust over the European block, moved back towards the Apenninic foreland to the SE, where they were superimposed on Units I and II, together with Unit III (Tuscan Nappe). In this way they provided the load necessary for the accomplishment of metamorphism in Units I and II.

As the deformation wave in the Apennines progressed east or southeast, the various Units were deformed and successively thrust over the adjacent basins to the SE.

The occurrence of internal deformation and metamorphism prior to overthrusting can account for the independence of metamorphism of the place occupied by the Units in the tectonic succession; this is also confirmed for the different ages found for metamorphism of Unit I and subunit IIa.

On the other hand deformation and metamorphism of the northern Apennines are only the latest events of a compressional history (most probably linked to the Corsica-Sardinia rotation), which must have begun some time earlier. Unless during the rotation the Corsica-Sardinia microcontinent moved as one block together with the Italian landmass (which was part of another plate) the rotation as described above must also have been connected to the consumption of the oceanic crust that was situated between Corsica-Sardinia and the Italian plate⁸. It is difficult to escape the conclusion that the folding of the northern Apennines is connected to the rotation of the Corsica microplate. Thus, metamorphism in the northern Apennines offers the opportunity for dating a late moment of the Corsica-Sardinia rotation.

Table 1 Dating of Metamorphic Muscovite Samples

Sample *	K %	ml. ⁴⁰ Ar 10 ⁻⁵ /gK	rad. ⁴⁰ Ar/ total ⁴⁰ Ar	Age m.y.
Unit I A1	5.77	4.43	0.44	11.0
A2	6.18	4.49	0.79	11.2
C1	7.85	4.70	0.64	11.8
Subunit IIa B1	3.96	6.07	0.60	15.2
B2	2.62	6.12	0.51	15.3
B3	5.45	6.81	0.74	17.0
B4	2.52	5.86	0.35	14.5
B5	3.32	4.83	0.41	12.1
B6	8.02	5.26	0.53	13.0
B7	4.70	6.25	0.43	15.3
Subunit IIb P1	6.03	4.15	0.53	10.8
P2	2.97	4.37	0.44	10.9

* Sample numbers differ from ref. 4.

We dated the metamorphic muscovite which is found at many levels in Units I and II, especially in impure marbles and phyllites: after dissolving the calcareous fraction in acetic acid the mica was separated from other minerals of the residue, quartz, pyrite, chlorite, sometimes kyanite and epidote. The muscovite was analysed for K and Ar in a routine way. The results are shown in Table 1; further details are given in ref. 4.

Metamorphism in subunit IIa ended at about 14 m.y.; in I and IIb it lasted until 11 m.y.; this is a confirmation of the tectonic conclusions (thrusting of the allochthonous Units after a phase of internal deformation) that could be reached from the fact that IIa's metamorphism is higher than I's.

Because the metamorphism was contemporary with internal deformation, the age of 14 m.y. in subunit IIa represents an upper limit for the beginning of compressional tectonic movements in the zone of northern Apennines and therefore for the start of the rotation of our microplate. The K/Ar method dates the end of the metamorphic reactions, so we must add to the 14 m.y. the time necessary for the metamorphism to take place.

The duration of single metamorphic episodes has been shown to have been between 2 and 10 m.y. in the Alps⁹. Even attributing to the metamorphism of subunit IIa the minimum duration

(2 m.y.), we must push back the beginning of the metamorphism to at least 16 m.y.

Further, it is doubtful that metamorphism can be considered the first tectonic event in the evolution of the northern Apennines; that is, it must have been preceded by other tectonic events. This is particularly significant in the case of subunit IIa, where metamorphism took place under a rather high P/T ratio, as shown by the presence of kyanite: the high pressure is to be attributed to a tectonic overcharge. Thus to the previously estimated limit of 16 m.y. more time must be added during which a series of compressional events preparatory to metamorphism took place. The age of 16 m.y. for the beginning of the rotation thus represents a lower limit; the upper limit remains undefined.

Regarding the rotation, the following considerations are of interest: with the Messinian (Pontian), distensive movements are recorded in the zone between Corsica-Sardinia and Tuscany. These movements, still active in some places, dissected the Tyrrhenian side of this mountain range in a series of horsts and grabens, and in a general way tended to increase the space between Corsica-Sardinia and Tuscany, thus acting in a sense contrary to the rotation. The rotation and the compression with it must have ended with the Messinian. As the Miocene-Pliocene limit in Tuscany is fixed at about 7 m.y., the end of the rotation must be set at least at 9–10 m.y.

This work was carried out while one of us (F. R. B.) was visiting at the Rosenstiel School of Marine and Atmospheric Science of the University of Miami, Florida, with a fellowship provided by the Italian CNR. We thank E. Bonatti and J. Honnorez, of the School, for helpful discussions and criticism.

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Received December 12, 1972.

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The Silica Budget of the Antarctic Circumpolar Current

THERE is disagreement as to whether the Antarctic Circumpolar Current is a major source of dissolved silica arising from the dissolution of glacier flour^{1–3} or a sink due to sedimentation of diatomaceous opal^{4,5}.

The distribution of silica in the World Ocean is greatly influenced by biological factors. It forms the tests of radiolaria and diatoms which grow mainly in the surface waters. The

resulting particulate phases, tissue, protoplasm and tests sink across the thermocline mixing-barrier into the deep waters: a variety of biological and inorganic mechanisms act to oxidize and redissolve a large fraction of this material both while it is sinking through the water column and while it is resting on the bottom. The net chemical effect of this process is to separate the biologically active and inactive dissolved components. The distribution of the latter is largely controlled by the mechanisms of primary injection, continental runoff, submarine weathering, precipitation and absorption, and the physical hydrodynamic processes of mixing and transport. Biological activity, however, acts to "strip" the required dissolved constituents from the surface layer and inject them into the deep layer. The return of both active and inactive components to the upper waters is by the same physical mechanisms.

The North Atlantic Deep Water (NADW) is formed from such "stripped" surface water which has been advected north from low latitudes⁶. Hence the concentrations of biologically active materials are extremely low in this water mass. On its southward flow the NADW encounters the eastward flowing Circumpolar Current at about 40° S after the latter's emergence from the Drake Passage. Geostrophic calculations⁷ indicate that the Deep Water flow across 30° S latitude in the Atlantic is approximately 18 Sverdrup ($\text{m}^3 \times 10^6 \text{ s}^{-1}$). Reid and Nowlin⁸ have recently determined the flux through the Drake Passage at 240 Sverdrup. The NADW is therefore almost completely assimilated in this massive current, its presence being indicated only by the broad salinity maximum in the Circumpolar Deep Water⁹.

The Circumpolar Current itself is driven by the prevailing Westerlies whose core is centred at 45 to 50° S (ref. 10). Close to the continent the prevailing winds are easterly and at the divergence large-scale upwelling of Circumpolar Deep Water occurs¹¹. A large fraction of this water flows east and north under the Westerlies until it encounters the warm, more saline sub-Antarctic surface water in a strong, stable, frontal region, the Antarctic Polar Front, characterized by a drop in surface temperature of several degrees in a few kilometres^{11,12}. It is immediately north of the Front in a region of intense mixing that the Antarctic Intermediate water is formed.

The Southern Ocean receives much more water from precipitation than it loses by evaporation, the excess being about 50 cm yr⁻¹ (ref. 13). The input of ice and melt-water from Antarctica is between 5 and 10 cm yr⁻¹ (refs. 14, 15). Because these waters have extreme properties relative to the upwelling Deep Water, it is possible to estimate the flux of this latter component from salinity balance calculations¹⁶.

An independent approach can be made using oxygen isotope data¹⁷. Evaporation and freezing result in quite small fractionation effects in oceanic surface waters while the progressive stripping of rain-water from weather systems leads to an increasing enrichment of the lighter isotope in the residual vapour. The general motion of weather systems is polewards from the areas of high evaporation in low latitudes so that the rain-water in the Circumpolar region has an $^{18}\text{O}/^{16}\text{O}$ ratio very different from the surface sea water. Defining $\delta^{18}\text{O} = (R_{\text{sample}}/R_{\text{standard}} - 1) \times 1,000$ where the δ values are in per thousand, $R = ^{18}\text{O}/^{16}\text{O}$ and the standard is Standard Mean Ocean Water, SMOW (ref. 18), the values for surface waters cluster around zero, for marine water vapour -10 to -14‰ (about 3‰ lighter than the equilibrium value¹⁷), and for high latitude precipitation -20 to -30‰. Craig and Gordon¹⁷ estimated the average $\delta^{18}\text{O}$ values for the various components of the water flux in the Antarctic Ocean (Table 1).

Assuming a balance between the flux of water and of oxygen isotopes into the mixed layer from the upwelling of Circumpolar Deep Water (V_{DS}) precipitation (V_{AS}) and continental ice and melt waters (V_{CS}) and the flux out as Intermediate Water (V_{SI}), Bottom Water (V_{SB}) and evaporation (V_{SA}) then

$$V_{\text{DS}} - V_{\text{SI}} - V_{\text{SB}} + V_{\text{AS}} - V_{\text{SA}} + V_{\text{CS}} = 0 \quad (1)$$

$$V_{\text{DS}} \delta_{\text{D}} - V_{\text{SI}} \delta_{\text{S}} - V_{\text{SB}} \delta_{\text{SB}} + V_{\text{AS}} \delta_{\text{AS}} - V_{\text{SA}} \delta_{\text{SA}} + V_{\text{CS}} \delta_{\text{C}} = 0 \quad (2)$$

Table 1 Average Values of Various Parameters of the Antarctic Ocean south of the Polar Front

		Reference
Area	$2 \times 10^{17} \text{ cm}^2$	16
Precipitation	100 cm yr^{-1}	13
Evaporation	50 cm yr^{-1}	13
Input of continental ice and melt water	$\begin{cases} 5 \text{ cm yr}^{-1} \\ 10 \text{ cm yr}^{-1} \end{cases}$	 14 15
	$S \text{ ‰}$	$\delta^{18}\text{O} \text{ ‰}$
V_{AS}	0	-23
V_{CS}	0	-30
V_{SA}	0	-4.0
V_{SI}	34.00	-0.45
V_{SB}	34.65	-0.45
V_{DS}	34.74	-0.21

where δ_D , δ_S and δ_C are the values for Deep, Surface and Continental melt water respectively, δ_{SB} for the sinking cold water and δ_{AS} and δ_{SA} for the rain-water and evaporative components.

Re-arranging (2)

$$V_{DS} \delta_D + V_{AS} \delta_{AS} + V_{CS} \delta_C = (V_{SI} + V_{SB}) \delta_S$$

because the surface water goes directly to form AAIW and as freezing has only a very small fractionation effect¹⁷. Substituting from (1) and re-arranging gives

$$V_{DS} = [V_{AS}(\delta_S - \delta_{AS}) - V_{SA}(\delta_S - \delta_{SA}) + V_{CS}(\delta_C - \delta_C)] / (\delta_D - \delta_S)$$

Evaluating in units of cm yr^{-1} of upwelling

$$V_{DS} = [100(-0.45 + 23) - 50(-0.45 + 4.0) + 10(-0.45 + 30)] / (-0.21 + 0.45) = (2,255 - 177.5 + 295.5) / 0.24$$

giving $V_{DS} = 9,887 \text{ cm yr}^{-1}$. This is equal to the depth of the mixed layer and represents an average upwelling rate of 63 Sverdrup in excellent agreement with the estimate of Gordon¹⁶. It follows that the residence time of water in the surface layer is of the order of 1 yr, and certainly between 6 months and 2 yr, because the uncertainty in $(\delta_D - \delta_S)$ cannot be greater than $\pm 0.1 \text{ ‰}$. This value for V_{DS} can be used in a salt-balance to estimate V_{SD} . For simplicity

$$S_{AS} = S_{SA} = S_{CS} = 0, \quad S_S = S_{SI}$$

and hence:

$$V_{DS} S_D - V_{SI} S_S - V_{SB} S_{SB} = 0$$

$$V_{SB} = [V_{DS}(S_D - S_S) - (V_{AS} - V_{SA} + V_{CS})S_S] / (S_{SB} - S_S) = (7,229 - 2,040) / (S_{SB} - S_S) = 5,189 / (S_{SB} - S_S)$$

This is a measure of the removal rate of water whose salinity has been sufficiently modified by freezing that it can sink through the pycnocline. If it is assumed that all sinking occurs in the Weddell Sea, that all the water reaches the bottom and that the mixing ratio with the Deep Water is 1 : 1, then the production rate of Bottom Water is: $(5,189/0.65) \times 2 = 101$ Sverdrup. This is a maximum value which, although in agreement with those of Wust¹⁹ and Le Pichon *et al.*²⁰, is about three times the value of most other estimates²¹. The important result of these calculations is the very high upwelling rates and correspondingly low residence times found for the surface waters south of the Polar Front.

Sverdrup *et al.*⁷ remarked on the striking lateral homogeneity of the Antarctic Circumpolar Current as indicated by sections of T, S and O_2 along its main axis, about 10° south of the Front. In striking contrast the oxygen levels at 2,500 m 15° north of the Front, for instance, are $235 \text{ } \mu\text{m kg}^{-1}$ in the South Atlantic and $195 \text{ } \mu\text{m kg}^{-1}$ in the South Pacific, the corresponding values for silica being $45 \text{ } \mu\text{m kg}^{-1}$ and $100 \text{ } \mu\text{m kg}^{-1}$. The average values in the Circumpolar Deep Water are $210 \text{ } \mu\text{m kg}^{-1}$ for oxygen and $110 \text{ } \mu\text{m kg}^{-1}$ for silica. From

this it can be deduced that there is no simple "source" of Circumpolar Deep Water. Superimposed on the general easterly flow is a southerly component necessary to maintain the upwelling which must occur all around the continent rather than at one or several discrete locations: in addition the rate of entrainment or percentage renewal per circuit must be sufficiently small that the general lateral homogeneity be maintained. Taking the transport south of the Polar Front as 240 Sverdrup (ref. 8) and the upwelling rate as 60 Sverdrup gives the percentage replacement as 25% in the average circuit time of 10 yr, that is an average residence time for water of 40 yr, consistent with the above deductions. While the general composition of the Circumpolar Current is probably controlled by this averaging of the inputs from all round the Southern Ocean, the high silica values suggest that significant chemical processes do occur within the Current itself. Large-scale, light-limited²² production of diatoms takes place in the nutrient rich upwelled water, leading to significant but highly variable surface depletions of dissolved silica. Edmond and Jacobs (unpublished data) found a range of from less than 10 to over $80 \text{ } \mu\text{m kg}^{-1}$ on Eltanin 47 (January to April 1971). Taking the average concentration of silica in the incoming water as $70 \text{ } \mu\text{m kg}^{-1}$, and in the Deep Water as $110 \text{ } \mu\text{m kg}^{-1}$, then with the 40 yr residence time an input of silica of $7.2 \times 10^{19} \text{ } \mu\text{m yr}^{-1}$ is required to maintain the Deep Water levels (mean thickness of the Deep Water taken as 3,600 m).

The published Eltanin data^{23,24} suggests an average silica concentration in the mixed layer of $60 \text{ } \mu\text{m kg}^{-1}$. If the depletion occurs by stripping of the mixed layer by diatoms then the input is $10^{20} \text{ } \mu\text{m yr}^{-1}$ (100 m mixed layer, residence time of 1 yr). This is sufficient to maintain both the high silica concentrations in the water column and the high sedimentation rate of diatoms all around the continent. Even these very crude calculations show clearly that the Antarctic diatomites represent a net sink for silica and that no local injection is required in order to maintain the steady state situation: a massive source of silica from glacial sediments is ruled out.

The now-classic problem posed by Sillen²⁵ concerning the equilibrium versus steady-state control of the silica and other cycles in the ocean is very difficult to answer directly. The stream supply of silica is estimated by Livingston²⁶ at $4.3 \times 10^{14} \text{ g SiO}_2 \text{ yr}^{-1}$ corresponding to $7.2 \times 10^{18} \text{ } \mu\text{m Si yr}^{-1}$. This is a factor of ten less than the estimated dissolution rate of diatomaceous opal in the Circumpolar Current. Bacon and Edmond²⁷ estimate the total dissolution rate of opaline silica at $4.4 \times 10^{18} \text{ g Si } 10^{-3} \text{ yr}$ corresponding to $1.6 \times 10^{20} \text{ } \mu\text{m Si yr}^{-1}$. We see that the net fluxes into and out of the oceanic reservoir of dissolved silica are very small compared to the internal fluxes of precipitation and dissolution of opal. The cyclic processes dominate over the primary ones. It is therefore not feasible to attempt to identify net fluxes of silica into various reservoirs by mass balance calculations.

I thank A. L. Gordon and S. S. Jacobs of Lamont-Doherty Geophysical Laboratory for support and encouragement. The Office of Polar Programs enabled my participation in Eltanin Cruises 34 and 47. The work was supported by the ONR.

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Received October 4; revised November 3, 1972.

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Equilibrium Cross-section of Meandering and Braided Rivers

THERE are two simple models which describe the equilibrium state of a channel cross-section with constant conditions, one static, the other dynamic. In the first case equilibrium is said to hold when all grains on the bed are just at the threshold of movement¹. Because movement is produced by a combination of fluid drag and the downslope gravity component and because the fluid drag is greatest in the centre of the channel, the channel slopes can increase up the sides to compensate for the decrease in drag. The resulting profile is roughly a parabola¹ and is very similar to many natural channel forms. This static model is particularly relevant to the design of some canals where discharges are fairly constant and the bed material is normally immobile. Natural rivers transport sediment, however, and the mean channel form develops at high stages when nearly all the bed material is in intermittent motion. When sediment is transported as bed load it must be continually tending to creep downslope into the centre of the channel under the influence of gravity. If, however, the channel maintains a constant profile there must be an equal and opposite tendency at all points for the sediment to move sideways upslope. Apart from buoyant upthrust, the only other force acting on the grains is the fluid drag acting in the direction of the water flow, leading to the second model. When river channels are in dynamic equilibrium the flow along the bed has a sideways component up the channel banks which exactly compensates the tendency of the sediment to move downslope under the influence of gravity. As water flowing sideways along the bed must return to the channel centre along the surface this implies that the river has a double spiral secondary flow (Fig. 1). Although double spiral flow in straight channels has been demonstrated^{2,3}, single spiral flow in meander bends is better known¹. As, however, the sense of the spiral changes from one bend to the next, it follows from considerations of continuity and symmetry that in the straight reaches between bends there is a transitional double spiral structure (Fig. 1 C). In short the secondary flow in river channels is a double spiral which is asymmetrically developed in bends so that one vortex momentarily excludes the other at the crest of the meander. The two spirals adjoin where the flow attaches itself to the bed and diverges sideways; this line is also the thalweg or deepest part of the channel. It is no coincidence that the same

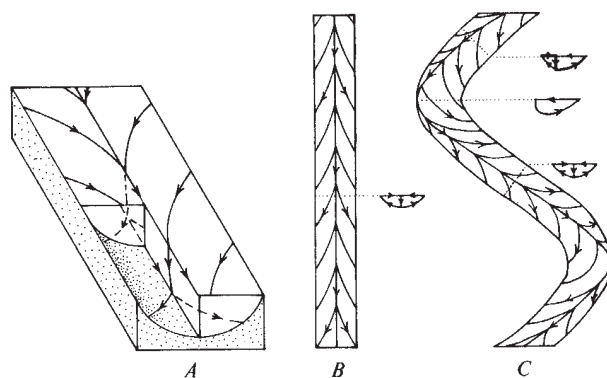


Fig. 1 Models for double spiral flow in channels. A, B, Symmetrical double spiral. In nature this is found at the cross-over on the straight reaches. C, Asymmetrical double spiral in a meandering channel. The pitch of the spiral is exaggerated to make the relations clearer.

hypothesis of double spiral flow is commonly used to explain the origin of many sand ridge bed configurations formed parallel to the flow, such as primary current lineation, sand ribbons, tidal current ridges and longitudinal aeolian dunes and draas^{4,5}. River channels are analogous to the troughs between longitudinal sand ridges in certain other respects too: they arise spontaneously during sediment transport, reach a dynamic equilibrium by interaction with the flow and are associated with transverse structures, forming meander bars, that migrate downstream. In short, there is a good case for saying that the entire form of a river channel is a bed configuration *sensu stricto*.

The long profile of a river changes slowly with the passage of time or in response to changes in climate or base level. In contrast the cross-sections of river channels change rapidly in response to changing conditions, particularly varying discharge. This is understandable because in the first case large quantities of material have to be moved hundreds of kilometres downstream, and in the second case only small quantities are involved and they need only be moved hundreds of metres laterally. The cross-section of a river tends to grow towards a constant equilibrium profile. Because the flow conditions in all rivers fluctuate rather rapidly such an equilibrium is never reached; however, it is quite sensible to consider the mean cross-profile of many rivers as being in an approximate equilibrium with the mean flow conditions. This is rarely true if they are cut in and restricted by bedrock, but it is probably true of most rivers flowing in unconsolidated alluvium. Furthermore, since the cross-profile adjusts much more rapidly than the long profile to changing conditions, the effect of changes in long profile in upsetting the mean equilibrium cross-section can be considered as negligible. All unrestricted channel thalwegs, whether in single or multichannel streams, have a tendency to be sinuous⁶. This sinuosity also tends to reach an equilibrium form dependent on the conditions and it also adjusts to changes in these much more rapidly than the long profile. Here I discuss only the mean channel cross-section and I make the simplifying assumption that the sinuosities are averaged out to form a single symmetrical straight channel.

Consider a river channel in equilibrium. At every point the downslope gravity component acting on a moving grain is balanced by the upslope component of the fluid drag, both forces acting normal to the channel axis. In given flow conditions, the forces acting on a single grain in a bed of mean grain diameter d are related to its size by

$$\begin{aligned}\text{Gravity component} &\propto d^3 \\ \text{Fluid drag component} &\propto d^2\end{aligned}$$

because the former is a body force and the latter a surface force. Consider exactly the same situation where the grain size is

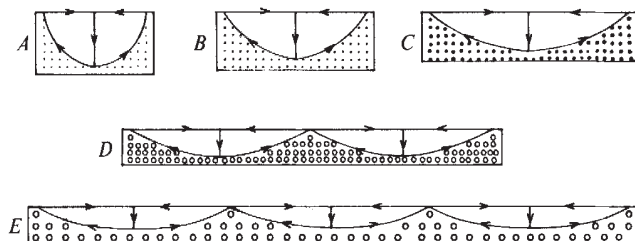


Fig. 2 Hypothetical differences in the equilibrium cross-profiles of rivers with the same slope and discharge but different bed grain sizes (increasing from A to E).

doubled. Neglecting the effect of the small change in bed roughness, the fluid drag per unit area remains the same and the upslope fluid drag on each grain is increased four-fold. However, the downslope gravity component per grain is increased eight-fold. This means that the profile is no longer in equilibrium and the grains move inwards from the sides to the centre of the channel, making the cross-section wider and shallower. As the side slopes become gentler the downslope component decreases until a new equilibrium is reached. If the grain size were halved instead of doubled the upslope fluid drag on each grain would be reduced to a quarter and the downslope force reduced to an eighth. In this case the lateral component of spiral flow would move sediment from the centre up the sides of the channel until a new equilibrium had been reached with a narrower, deeper, steeper sided cross-section. This shows that, other things being equal, rivers with coarse grained beds are wide and shallow and rivers with fine grained beds are narrow and deep (Fig. 2). An important corollary of this is that the coarsest material gravitates to the centre of the channel, not because the fluid drag is greater there but because the side slopes are gentler.

As the channel changes so must the shape of the spiral vortex within it. This accommodation is unlikely to continue indefinitely, however, because the secondary flow has its origin in hydrodynamic instability and the causal factors involved do not include the forces acting on individual sediment grains. In fact, the occurrence of the spiral flow is probably independent of whether the bed is fixed or mobile and of whether the water is restricted to a channel or flows in a wide sheet. It would be reasonable to expect the width of unrestricted spiral vortices to increase with deepening water and *vice versa*, because the dimensions of natural entities usually increase or decrease together even if their relative proportions do not remain the same. If the water depth is decreased with constant width or the width increased with constant depth, sooner or later the secondary flow will break up into two and subsequently many spiral pairs. This has been demonstrated experimentally⁶⁻⁸. Each of these spiral secondary flow systems should form a sub-channel in the river. The foregoing argument suggests that a change of grain size affects the equilibrium cross-profile of a river in the way shown in Fig. 2. With increasing grain size the channel becomes shallower and wider until a stage is reached where the secondary flow divides to form two vortex pairs with two sub-channels and subsequently many vortex pairs with many sub-channels. This hypothesis explains the origin of braided multichannel rivers. Other things being equal, braided rivers will form with coarse grained beds and meandering rivers with fine grained ones.

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Received May 25, 1972.

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The Racemization Reaction of Isoleucine used as a Palaeotemperature Indicator

It has been shown that the slow racemization reaction of amino-acids can be used in geochronology¹⁻⁴. One reaction of particular interest is that involving isoleucine. The racemization of L-isoleucine (iso) produces the non-protein amino-acid D-alloisoleucine (alleu), and these two can be separated on an automatic amino-acid analyser. The isoleucine racemization reaction in bone has a half-life at 20° C in excess of 100,000 years and preliminary evidence suggests that the reaction can be used to date fossil bones too old to be dated by radiocarbon⁴.

The ages based on the racemization of isoleucine are calculated from the equation (see refs. 3 and 4 for derivation):

$$\text{"age" (years)} = \frac{\text{Ln} \left[\frac{1 + (\text{alleu/iso})}{1 - 0.725 (\text{alleu/iso})} \right] - 0.028}{(1.725)(10^{19.41 - 7304.0/T})} \quad (1)$$

where T is the average temperature (K) of the bone since deposition. It is evident from this equation that because of the sensitivity of the racemization reaction to temperature, some estimate of the temperature history of the sample must be available. Conversely, if the age of a bone can be determined by an alternative method, the racemization reaction can be used to calculate the average temperature which a bone has experienced since it was deposited. Thus, under certain conditions, the racemization reaction could be used as a palaeothermometer; we now describe an example of such a use of the reaction.

We analysed samples from the site of Florisbad, near Bloemfontein, Orange Free State, South Africa. The bones were from a peat layer (Peat I) which was next to the "eye" of a warm lithium spring. Peat I is the lowest of several peat layers and contained hominid and faunal remains and several wooden implements.

The bones used in these investigations were from an extinct hippopotamus (*Hippopotamus amphibius*). Radiocarbon analysis of the collagen fraction isolated from a mandible yielded an age of $38,680 \pm 2,000$ yr (UCLA-1745B); the radiocarbon age of a wooden implement associated with the jaw bone was determined to be $38,550 \pm 3,800$ yr (UCLA-1745C). In 1932 the fragmentary skull (facio-calvarial fragments) of *Homo (Africanthropus) helmei* was recovered from the Florisbad site⁵. These hominid remains can now be dated by the radiocarbon ages of the hippopotamus and wooden implements. Organic matter from the peat was used by several laboratories to date this site⁶⁻⁹; our dates are the first on individual fossil or cultural materials. The archaeological and anthropological implications of the new radiocarbon ages are discussed elsewhere¹⁰.

Amino-acid analyses were carried out on a piece of the hippopotamus jaw bone and also on a portion of a tooth associated with the jaw. The samples were processed and analysed according to the procedure described elsewhere⁴. We found the alleu/iso ratios to be 0.46 (bone) and 0.42 (tooth).

Substituting these ratios and the radiocarbon age into equation (1) yields a temperature of $26.5 \pm 0.3^\circ \text{C}$. The present average temperature in the spring is about 28°C (private communication from H. Oberholzer), which agrees well with the value deduced from the isoleucine racemization reaction. Moreover, because the analysed materials were not from the spring itself, but were from the adjacent peat layer, the calculated and actual temperatures of the samples are probably even in closer agreement.

It should be emphasized that a temperature calculated from the amino-acid racemization reaction is the average temperature to which the bone has been exposed. The fact that the calculated temperature of $\sim 27^\circ \text{C}$ for the spring so closely matches its present temperature suggests that there have been no large scale temperature fluctuations of long duration; the spring temperature has apparently been nearly constant for a period of at least 40,000 yr. By contrast, because of temperature variations during glacial periods, the average temperature for the surface waters of the tropical Atlantic Ocean during the past 40,000 years is $\sim 24^\circ \text{C}$, almost 3°C lower than the present ocean temperature¹¹. It has also been estimated², from the oxygen isotopic composition of two speleotherms, that the inland regions of New Zealand have experienced a similar lower average temperature during the past 40,000 yr. This 3°C decrease in average temperature is not seen in the Florisbad warm spring. Either the spring temperature has been unaffected by climatic variations in the surrounding environment or, during the past 40,000 years, the temperature fluctuations in South Africa were much smaller than elsewhere on the Earth.

We thank Dr Hannes Oberholzer (Director, National Museum, Bloemfontein) for providing material, and Dr Rainer Berger (University of California, Los Angeles) for helpful advice. This research was supported by the Petroleum Research Fund which is administered by the American Chemical Society. The radiocarbon dates were determined at UCLA and were supported by a grant from the National Science Foundation. The amino-acid analyser was obtained through a grant from the Oceanography Section of the National Science Foundation.

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BIOLOGICAL SCIENCES

The Unexpected Location of a Gene conferring Abnormal Radiation Sensitivity on Phage T4

THREE mutations have been characterized, each of which increases the radiation sensitivity of phage T4 by a factor of about two. The ν gene, controlling excision repair, maps close to gene 1 which codes for hydroxymethyl-dCMP kinase, a precursor of phage DNA synthesis¹⁻³. The x gene, which probably is involved in a post-replication repair system, maps close to gene 43, the gene coding for the phage Kornberg polymerase^{1,3,4}. The third gene, y , affects radiation sensitivity in a similar manner to x ⁵. It has not previously been mapped, although it has been shown to be unlinked both to ν and to x ⁴. We have now located the y gene on the circular genetic map of phage T4. Unlike ν and x which as expected map close to known early genes involved in phage DNA synthesis, the y gene is surrounded by late genes which determine the structure and assembly of the phage head and tail (Fig. 1).

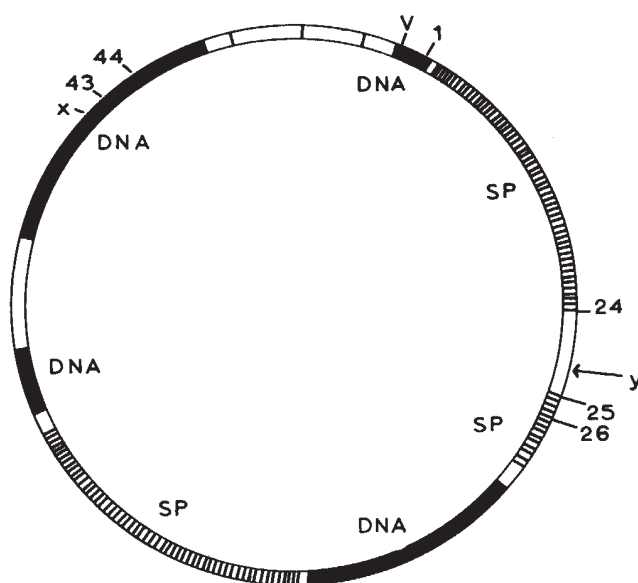


Fig. 1 Simplified circular genetic map of phage T4 (after Edgar and Wood⁶) showing the locations of the genes mentioned in the text. Genes known to affect DNA synthesis all lie in the portions of the genome marked "DNA". With very few exceptions, genes known to be involved in the synthesis and assembly of phage structural proteins lie in the portions marked "SP".

The original y mutant was isolated from a phage stock which had been heavily mutagenized with hydroxylamine, and then back-crossed four times with T4 wild-type. The resultant phage variant, called y_{10} , showed an amber phenotype in that its ultraviolet sensitivity was normal when assayed on su^+ bacteria but was enhanced when assayed on su^- bacteria; however, it plated equally well on both types of bacterial strain. On performing further back-crosses with y_{10} we observed a great heterogeneity in the plaque-types among the phage lysates suggesting that y_{10} was still a multiple mutant. After a further series of back-crosses we finally isolated a phage variant, termed y_{100} , which now behaved like an essential amber mutant having an efficiency of plating of less than 10^{-3} on su^- bacteria as compared to its titre on su^+ hosts. The rare plaques arising from y_{100} when plated on su^- bacteria were shown to be due to pseudo-revertants which now had the same radiation properties as y_{10} and genetically could be analysed as double-

mutants containing y_{100} coupled with suppressor mutations which map at a number of sites on the T4 genome.

Having now isolated as an essential amber mutation the ingredient in y_{10} which determined its radiation sensitive properties, it was comparatively straightforward to map y_{100} by conventional 2-factor crosses with known amber mutants. This procedure gave no indication of linkage between y_{100} and genes in the known early regions of the genome but did indicate a definite linkage with the late genes 24, 25 and 26, recombination frequencies being 18%, 8% and 12% respectively. To locate the y_{100} mutation more accurately with regard to these genes a series of 3-factor crosses of the type $am24-y_{100} \times am25$ was performed. All the plaques obtained by plating the progeny on su^+ indicator were then analysed and the ratio of y^+/y phage determined among the am^+ recombinants. The results are presented in Table 1, together with those of a 2-factor cross between y_{100} and $am25$ analysed in the same non-selective manner. The only interpretation of these results which is consistent with the observation that the mutations used in genes 25 and 26 are about 8 map units apart is that the y gene lies between genes 24 and 25.

Table 1 Non-selective Analysis of 2 and 3 Factor Crosses of the Type $am \times y$ and $am-y \times am$

Type of cross	Total	Plaques scored am^+y^+	am^+y	am^+y^+/am^+y
24y × 25	1,078	15	112	0.13
24 × 25y	1,006	128	26	4.92
25y × 26	1,527	75	16	4.69
25 × 26y	1,419	11	53	0.21
			Recombination frequency	
25 × y	1,072	41		7.6

The amber mutants used were B26 (gene 24), N67 (gene 25) and N131 (gene 26). Crosses were carried out in su^+ host bacteria and the progeny then plated, also on su^+ bacteria. In the 3-factor crosses individual progeny plaques were first tested by complementation against the appropriate double amber mutant, and the y phenotype of all am^+ recombinants was identified by examining both their relative plating efficiencies and ultraviolet sensitivities on su^+ and su^- indicator. A similar procedure was employed in analysing the 2-factor cross.

The present results are of interest for two reasons. First, they have identified a new essential gene in phage T4 which, because it is implicated in radiation repair, is presumably involved in some aspect of phage DNA synthesis. Second, they suggest that the previously uncharted region of the T4 genome which lies between genes 24 and 25 (and is approximately 20 map units in length) represents a further region which contains genes involved in early functions.

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Thymus and Bone Marrow Derived Lymphatic Leukaemia in Mice

VERY little information is available concerning the origin of the lymphoid cells of human and animal leukaemias. The lymphoid cell population has been divided into two different functional groups, namely the thymus derived lymphocytes (T cells) mainly involved in cell mediated immunity, and the bone marrow derived lymphocytes (B cells) mediating humoral immunity. These two lymphocyte categories have not been distinguished morphologically, but several specific markers for their identification have been described. The thymus, brain and thymus derived lymphocytes (T cells) bear a cell surface isoantigen θ^1 and have very little detectable immunoglobulin molecules on their surface; whereas the bone marrow derived lymphocytes (B cells) have a high density of immunoglobulin determinants on their surface².

Several examples of B lymphocyte leukaemias have been reported. Among human leukaemias and lymphomas several cases of chronic lymphatic leukaemia³⁻⁵ and Burkitt lymphoma⁶ were described as having surface immunoglobulins on the lymphocyte cell membrane, therefore representing a B-lymphocyte malignancy. Similarly, a cell passage line of a spontaneous occurring leukaemia in guinea-pigs was shown to have multiple properties characteristic of bone marrow derived B cell population⁷.

It has been established that radiation and virus induced lymphatic leukaemia in mice are usually dependent on the presence of the thymus which is probably the target organ for the actual neoplastic transformation⁸. By contrast, we found that the induction of lymphatic leukaemia in the inbred SJL/J strain of mice by a chemical carcinogen was not thymus dependent, as we obtained a high incidence of lymphatic leukaemia in thymectomized mice treated with the carcinogen⁹. We thought it of interest to analyse the lymphocyte origin of radiation, virus and chemical carcinogen induced lymphatic leukaemias and lymphosarcomas.

We chose for these studies the SJL/J strain of mice which were previously shown to be susceptible to lymphatic leukaemia induction by chemical carcinogens, fractionated irradiation, or by a virus isolated from isogenic irradiated non-leukaemic bone marrow^{9,10}. Our first attempt was to induce lymphatic leukaemia by a chemical carcinogen in mice devoid of T cells. It has been shown that injection of isologous bone marrow to thymectomized irradiated adult mice prevents thymus derived lymphocyte differentiation from the injected bone marrow, and bone marrow derived lymphocytes persist as the predominant lymphocytes in the lymphoid tissues¹¹. This method was used to obtain SJL/J mice with a bone marrow derived lymphocyte population in their lymphoid tissues. The carcinogen 7,12-dimethylbenz(a)anthracene (DMBA) was used for tumour induction. The carcinogen was made up as a 1% solution in polyethylene glycol 400 and administered to the mice four times at weekly intervals by stomach tube prepared from polyethylene tubing of about 1 mm bore size. A dose of 1 ml. of the DMBA solution (1 mg DMBA) was given at each feeding.

Five week old female SJL/J mice were thymectomized and thereafter divided into three groups. One group was treated with DMBA, starting 6 weeks after thymectomy was performed in these mice. Thymectomized mice of the second group were exposed to 550 R whole body irradiation and 2 weeks thereafter treated with DMBA. The third group was exposed 4 weeks after thymectomy to 800 rad whole body irradiation and within 1 h after irradiation they were reconstituted with 10⁶ isologous normal bone marrow cells and two weeks later treated with DMBA. The leukaemogenic potency of DMBA treatment was also evaluated in intact female SJL/J mice treated with carcinogen when 11 weeks old. Lymphatic leukaemia developed in all the different treated groups, the incidence ranging between 52-70% at an average latent period of 130-160 days (Table 1). A low incidence of stem cell leukaemia,

Table 1 Lymphatic Leukaemia Induction in SJL/J Mice by 7,12-Dimethylbenz(a)anthracene (DMBA)

Treatment of host	Lymphatic leukaemia incidence	Stem cell leukaemia incidence	Myeloid leukaemia incidence	Reticulum cell sarcoma incidence
Thx* $\xrightarrow{6 \text{ weeks}}$ DMBA $\times 4$	14/27-52% (160)	4/27-15% (136)	1/27-3.7% (170)	7/27-26% (242)
Thx $\xrightarrow{4 \text{ weeks}}$ 550 R W B $\xrightarrow{\dagger 2 \text{ weeks}}$ DMBA $\times 4$	23/33-70% (142)	3/33-9% (156)	2/33-6% (110)	4/33-12% (270)
Thx $\xrightarrow{4 \text{ weeks}}$ 800 R W B $\xrightarrow{1 \text{ hour}}$ BM $\xrightarrow{\dagger 2 \text{ weeks}}$ DMBA $\times 4$	12/20-60% (132)	4/20-20% (160)	1/20-5% (140)	1/20-5% (170)
Intact $\xrightarrow{11 \text{ weeks}}$ DMBA $\times 4$	15/28-52% (154)	4/28-14% (174)		5/28-18% (213)

Female SJL/J mice were thymectomized at the age of 5 weeks. DMBA treatment: four feedings of 1 mg in polyethylene glycol 400 administered by stomach tube at weekly intervals.

The average latent period in days is given in parentheses.

* Thymectomy. $\dagger$ Whole body irradiation. $\ddagger$ 10^6 isologous normal bone marrow cells injected intravenously.

myeloid leukaemia and spontaneous reticulum cell sarcoma was also observed (Table 1). This demonstrated the ability to induce lymphatic leukaemia by a chemical carcinogen in mice devoid of thymus derived lymphocytes, namely leukaemias that originate from a bone marrow derived population of lymphocytes.

Thymus dependent lymphatic leukaemia was induced by irradiation or by a leukaemogenic virus. Five week old SJL/J mice were exposed to four weekly doses of 170 R whole body irradiation. Thymic lymphomas occurred in 40% of the irradiated mice at an average latent period of 200 days.

The leukaemogenic virus used in the studies for thymic lymphoid leukaemia induction was prepared from thymic lymphoid leukaemias induced in SJL/J mice by live serial passage of the virus originally isolated from irradiated non-leukaemic bone marrow as previously described^{10,12}. The cell free extract (0.02 ml.) was injected directly into the adult thymus, and 2 days later the inoculated mice were exposed to 400 R whole-body irradiation¹³. Thymic lymphoid leukaemia developed in 70% of the inoculated mice at an average latent period of 150 days.

The markers which characterize T and B cells were used to identify the lymphocyte origin of the above mentioned induced lymphatic leukaemias. The detection of the θ antigen, which is present on the bulk of thymus lymphocytes and on thymus derived lymphocytes, served to establish the percentage of thymus derived lymphocytes in the various induced leukaemias. The evaluation of θ bearing leukaemic cells¹ was carried out on cell suspensions from DMBA, radiation or virus induced leukaemias (cells taken from primary tumours or from first to third cell passage material) as well as on thymus and lymph node cells taken from 2 month old normal SJL/J mice. Anti θ C₃H serum was prepared according to the method of Reif and Allen¹⁴. Two month old AKR mice were immunized with C₃H/eb thymocytes; 10×10^6 thymocytes were injected intraperitoneally at weekly intervals for 7 weeks. The mice were bled one week after the last thymocyte inoculation. Each serum preparation was titrated against normal mouse thymocytes; in most cases it was diluted 1:4 with saline. Normal AKR serum was used as control. Cell suspensions of various leukaemias and normal thymus were prepared in cold in minimal 'Eagle's medium' (MEM) containing 0.1% BSA. The suspensions were washed twice and adjusted to $5-10 \times 10^6$ cells ml.⁻¹. To 0.1 ml. of cell suspension, 0.1 ml. of anti C₃H serum was added. The mixture was incubated for 15 min at 37° C. After washing once with MEM-BSA, 0.1 ml. of guinea-pig complement was added and the mixture was incubated for another 30 min at 37° C. Cytotoxicity was estimated by the dye exclusion of trypan blue in saline. The controls used were cells in normal AKR serum (1/4); cells in

antiserum without complement. The θ antigen was detected in 70% of the lymphocytes taken from radiation or virus induced leukaemias. In contrast only 15% of the lymphocytes from leukaemias induced in intact mice treated with DMBA carried θ antigen and 4.3% of lymphocytes obtained from thymectomized irradiated DMBA treated leukaemia mice (Table 2).

Table 2 Surface Antigens in DMBA, Radiation and Virus Induced Lymphatic Leukaemia in SJL/J Mice

Type of cell tested	No. of mice	Mean % θ bearing lymphocytes	IgG, IgM immunoglobulins
DMBA induced leukaemia in intact mice	10	15% (0-35%)	++++
DMBA induced leukaemia in thymectomized irradiated mice	7	4.3% (0-14%)	++++
Virus induced leukaemia	7	70% (58-88%)	---- $\pm$
Radiation induced leukaemia	5	75% (62-87%)	----
Normal thymus	5	77% (66-85%)	----
Normal lymph nodes	3	27% (25-30%)	++

2 month old female SJL/J mice were used. DMBA treatment: four feedings at weekly intervals of 1 mg in polyethylene glycol 400. Virus induced lymphatic leukaemia: 0.02 ml. of the cell free extract was injected directly into one thymus lobe of the mouse, which was further exposed 2 days later to 400 R whole body irradiation. Radiation induced lymphatic leukaemia: four doses of 170 R whole body irradiation at weekly intervals.

Immunoglobulin determinants on the surface of lymphoid cells were detected using the direct immunofluorescent technique^{2,15}. Living cells from the different induced leukaemias were washed in PBS containing 1% gelatin and then exposed to fluorescein conjugated anti-mouse immunoglobulin reagents (anti IgG and anti IgM). After incubation for 30 min at 37° C the cells were washed three times in PBS-1% gelatin and resuspended in 50% glycerin and stored at 4° C. As the cells were alive during the procedure the antibodies could only react with cell surface immunoglobulins. Surface staining was found only in lymphocytes from DMBA induced leukaemias (Table 2). The intensity and distribution of the staining were similar among the leukaemic lymphocytes taken from intact or thymectomized irradiated hosts treated with DMBA. It had a striking distribution and many of the labelled cells showed a defined fluorescent "cap" at the cell surface.

Thus the DMBA induced lymphatic leukaemia in intact or thymectomized irradiated hosts appeared to be of B lymphocyte origin whereas the radiation and virus induced lymphatic leukaemia was of T lymphocyte origin. These findings are not specific for the SJL/J strain of mice. Preliminary studies in our

laboratory concerned with the lymphocyte origin in lymphatic leukaemia induced in the C57BL and C₃H strains of mice indicate that radiation and virus induced leukaemia in these strains of mice were also of T lymphocyte origin, whereas all DMBA induced leukaemias so far tested were found to be derived from bone marrow lymphocytes. Our results differ from those obtained by Shevach *et al.*¹⁶, who analysed 21 mouse leukaemias, including some chemical carcinogen induced cell lines, and found that none of them could be classified as B lymphocyte leukaemias.

This work was supported by a grant from the Talisman Foundation Inc., New York.

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Co-carcinogenic Effect of Progesterone on 20-Methylcholanthrene Induced Cervical Carcinoma in Mice

THE secretion of mucosubstances in the genital tract seems to be related to the ovarian output. It is well known that ambivalent cells of the vaginal squamous epithelium keratinize under oestrogenic action and mucify when progesterone acts synergistically with oestrogen. This last condition seems to be required, at pro-oestrus, a few hours before ovulation. Direct evidence of oestrogen secretion at early pro-oestrus^{1,2} and of progestin surge at mid-pro-oestrus in mice³ and in rats⁴ has been demonstrated.

At pro-oestrus, however, mucosubstances appeared in the cells of the genital tract of control C57BL6 mice. In this experiment we have shown that these mucosubstances were histochemically different. Acid and carboxyl mucosubstances were secreted by vagina and exocervix, but the uterine columnar epithelium secreted neutral mucosubstances, while the endocervical mucosal epithelium did not produce any glucidic secretion. In the course of this study, mucosubstances were systematically looked for in the squamous epithelium and were shown by the following histochemical reactions: PAS, salivary amylase, alcian blue (pH 2.6 and 1).

Progesterone administered for 9 weeks by subcutaneous implantation of pellets with a dose of 15 mg/mouse/3 weeks, to 25 C57BL6 3-month-old mice induced a strong intra-cellular reaction of mucification along the vaginal mucosa to exocervix. In the endocervical squamous epithelium, acid and

carboxyl mucosubstances appeared only in few foci of cells. As, however, no glucidic secretion was exhibited in the endocervical cells during the normal oestrous cycle, it can be concluded that parenteral administration of progesterone induced a mucin-producing metaplasia in the endocervical mucosa.

Local exposure of carcinogen, 20-methylcholanthrene (MC), in the cervical canal for 9 weeks, in 50 C57BL6 3-month-old mice induced one invasive carcinoma in the vagina-exocervix and five in the endocervix.

Parenteral administration of progesterone to 50 C57BL6 mice for 9 weeks at the dose mentioned above was associated with carcinogenic action and significantly increased the total number of invasive squamous carcinoma in vagina-exocervix and in endocervix, as shown in Table 1.

Table 1 Histological Types of Invasive Squamous Cell Carcinoma Appearing in the Vagina-Exocervix and Endocervix after Local Application of 20-Methylcholanthrene (MC) with and without Progesterone (P) after 9 Weeks Treatment

Invasive squamous cell carcinoma	Vagina-exocervix			Endocervix		
	MC	P	value	MC	P	value
Immature microcarcinoma	0	9	0.01	3	9	n.s.
Keratinized	0	1	n.s.	1	0	n.s.
Mucin-secreting	1	5	n.s.	1	21	0.01
Total number of invasive squamous cell carcinoma	1	15	0.01	5	30	0.01

Histochemical and histological examination of a hundred specimens under study showed that in our experimental conditions progesterone selectively influenced the maturation of the induced endocervical invasive carcinoma, gave a muco-epidermoid type ($P < 0.01$) and had no effect on the maturation of induced invasive carcinoma from the vagina and the exocervix which remained immature.

Our results would indicate that endocervix in the strain of mice used might be a target organ for the action of progesterone or for its metabolites.

The progestative hormone, by inducing a mucin-secreting metaplasia in the target organ, could make it sensitive to the action of the carcinogen, and would thus influence the histological type of the induced carcinoma. The role of progesterone on the carcinogenesis of the cervix has received very little attention^{5,6}. Kaminetzky⁷ noted that progesterone administration advanced cervical dysplasia induced by 20-methylcholanthrene and Glücksmann and Cherry⁸ drew attention to the fact that progesterone treatment did not affect the total number of induced tumours but increased the incidence of adenocarcinoma secreting mucus. Our findings suggest the possibility that a prolonged and high dose of progesterone or progestins which are known to provoke a strong stimulation of human endocervical glands^{9,10} could promote carcinogenic activity of the agent primarily responsible for cervical tumours in women and tend to favour selectively the development of the mucoepidermoid maturation of the cancerous cells.

This work was supported by a grant from the French national foundation Ligue Nationale Française Contre le Cancer.

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Cholera Enterotoxin: Failure of Anti-inflammatory Agents to Prevent Cyclic AMP Accumulation

THERE is considerable evidence that cholera diarrhoea is caused by accumulation of cyclic AMP in intestinal mucosa, stimulated by cholera enterotoxin¹⁻³. Prostaglandin-E₁ (PGE₁) reproduces the effects of cholera enterotoxin on both cyclic AMP and secretion of fluid and electrolytes by intestinal mucosa³ and it was suggested⁴ that the toxin might first act by stimulating the release or synthesis of prostaglandin which then acts through cyclic AMP. Bennett⁴ proposed that acidic anti-inflammatory drugs, as they are potent inhibitors of prostaglandin synthesis in several tissues⁵⁻⁷, might antagonize the effect of cholera enterotoxin. Jacoby and Marshall reported that indomethacin, aspirin, and several other anti-inflammatory agents inhibited the cholera toxin-induced accumulation of fluid in ileal-ligated rats⁸. Because of its potential therapeutic significance, I have studied this proposed mechanism.

The cyclic AMP system is stimulated by cholera enterotoxin in virtually every tissue so far tested, including liver, adipose tissue, and blood platelets, in addition to intestinal mucosa^{1,9-11}. Low concentrations (1–10 ng ml.⁻¹) of the purified toxin increased adenyl cyclase activity and cyclic AMP accumulation in human leucocytes^{12,13}, and this effect was similar in all respects to that on intestinal mucosa, including toxin concentration required, the characteristically delayed time-course, and specific inhibition by cholera toxoid and a canine antitoxin. The leucocyte seemed a suitable tissue for testing Bennett's suggestion *in vitro*, as PGE₁ also stimulates leucocyte cyclic AMP accumulation¹³⁻¹⁵.

Leucocytes of three healthy human volunteers were prepared as described previously and suspended in a Tris-buffered salt solution containing 0.3% (w/v) human serum albumin¹⁴. Leucocytes (1 × 10⁷ ml.⁻¹) and purified cholera enterotoxin 10 ng ml.⁻¹, a maximally effective concentration¹² were incubated for 90 min at 37° C. Varying concentrations of indomethacin or aspirin were added to leucocytes 5 min before addition of cholera toxin. Incubation was terminated by centrifugation (2,000g for 1 min), after which the supernatant fluid was discarded and the cell button resuspended in ice-cold 5% trichloroacetic acid. Leucocyte cyclic AMP was then assayed by the competition-binding method of Gilman¹⁶, modified as described previously¹⁵.

Table 1 Leucocyte Cyclic AMP after Exposure to Anti-inflammatory Agents and Cholera Enterotoxin

	Cyclic AMP, picomol per 10 ⁷ cells *		
	Exp. 1	Exp. 2	Exp. 3
No drug	4.4	7.2	5.6
Indomethacin 70 µg ml. ⁻¹	5.0	10	7.4
Aspirin 700 µg ml. ⁻¹	—	9.0	8.0
Cholera enterotoxin 10 ng ml. ⁻¹	39	75	45
+ Indomethacin 70 µg ml. ⁻¹	41	84	44
+ Indomethacin 7 µg ml. ⁻¹	38	69	43
+ Indomethacin 0.7 µg ml. ⁻¹	37	72	44
+ Aspirin 700 µg ml. ⁻¹	—	76	39
+ Aspirin 70 µg ml. ⁻¹	—	73	40
+ Aspirin 7 µg ml. ⁻¹	—	67	43
+ Aspirin 0.7 µg ml. ⁻¹	—	68	42
PGE ₁ 1 × 10 ⁻⁵ M	32	50	40

* Each value is the mean of duplicates differing by ± 8%.

Neither aspirin nor indomethacin produced any detectable inhibition of the eight-fold increase in leucocyte cyclic AMP caused by cholera enterotoxin (Table 1). PGE₁ alone caused accumulation of cyclic AMP, as described previously¹³⁻¹⁵. The maximal concentrations of both anti-inflammatory agents were considerably higher than those which produced complete inhibition of prostaglandin synthesis in guinea-pig lung⁵ and canine spleen⁷; 3–4 µg ml.⁻¹ for indomethacin and 30–40 µg ml.⁻¹ for aspirin.

The similarities between cholera enterotoxin's effects on the cyclic AMP content of gut mucosa and leucocytes suggest that the toxin's mechanism of action may be similar in the two tissues. If so, high concentrations of aspirin and indomethacin would not be expected to prevent accumulation of cyclic AMP in gut mucosa exposed to maximally effective doses of cholera enterotoxin. This possibility must be tested directly with maximally—and submaximally—effective doses of cholera toxin on the gut mucosa, and it must be shown in both leucocytes and mucosa that indomethacin and aspirin greatly reduce prostaglandin synthesis. The present results show that cholera toxin alone can increase cyclic AMP levels maximally in human leucocytes. They do not exclude the possibility that the gut behaves differently or that a dual mechanism exists. In addition to the direct action of toxin there might be an indirect effect involving prostaglandins, and this would become evident only in experiments with the submaximal doses of toxin. Nevertheless, increased synthesis of prostaglandin is unlikely to prove a universal mechanism for the actions of cholera enterotoxin, since the toxin (like cyclic AMP) stimulates lipolysis in adipocytes⁹, cells in which PGE₁ inhibits both cyclic AMP accumulation and lipolysis^{17,18}.

These results do not preclude a beneficial therapeutic effect of acidic anti-inflammatory agents in cholera. The inhibition of toxin-induced intestinal fluid accumulation by these agents *in vivo*⁸ deserves further investigation, even if it belongs to a class of drug effects in which cyclic AMP plays no role.

I thank Sally Dion for technical assistance, and the United States Public Health Service and the American Heart Association for support. The purified toxin was obtained from Dr W. B. Greenough, III.

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Received September 11, 1972.

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Development of K^+ -Conductance and Membrane Potentials in Unfertilized Sea Urchin Eggs after Exposure to NH_4OH

THE fertilization of the sea urchin egg starts a sequence of changes in the cell membrane which can be measured as changes of membrane potential and membrane resistance¹⁻³. The immediate consequence of contact of ovum and spermatozoon is an action potential, a depolarization and repolarization including a transient reversal of polarity. The overall sequence^{1,2} converts the anion-permeable membrane of the unfertilized egg (with a potential of about -10 mV, inside negative) to a rather typical K^+ permeable membrane (about -60 mV, inside negative). The same results are obtained with parthenogenetic activation of the egg.

We now find that exposure of unfertilized eggs to seawater containing NH_4OH at pH about 9 changes the properties of their membranes to those of fertilized eggs. In the terminology of our early work (see Fig. 1, curve A), the treatment induces the phase III events (the development of K^+ -conductance and a potential of -60 mV) while bypassing the earlier events normally associated with the action of spermatozoa or parthenogenetic agents. The eggs remain unfertilized and can be fertilized later.

Unfertilized eggs of *Lytechinus pictus* were placed in normal seawater (pH 8.1–8.2) to which was added sufficient NH_4OH to bring the pH to 9.0–9.1, about 1 mmol of NH_4OH/l . An alternative method was to make the seawater 5–10 mM in NH_4Cl and to titrate it to pH 9.0–9.1 with NaOH. The methods for holding and penetrating the cells and for measuring potentials and resistances were those described earlier^{1,2}.

Fig. 1 shows the changes in potential in each case. After normal fertilization (Fig. 1A) there is a phase I action potential, a phase II pause and a phase III development of the final potential of the fertilized egg. After exposure to NH_4OH seawater (pH 9.1) (Fig. 1B) the action potential is bypassed and the unfertilized egg enters and completes the phase III events. The effects of increasing the external K^+ concentration at the end of the experiment indicate that the potential is a K^+ concentration potential. After exposure to seawater brought to pH 9.1 with NaOH (Fig. 1C) the phase III events are induced, but only after a long time.

We interpret the development of the phase III potentials in the unfertilized egg in NH_4OH seawater as the consequence of an increase in the internal pH. The much slower develop-

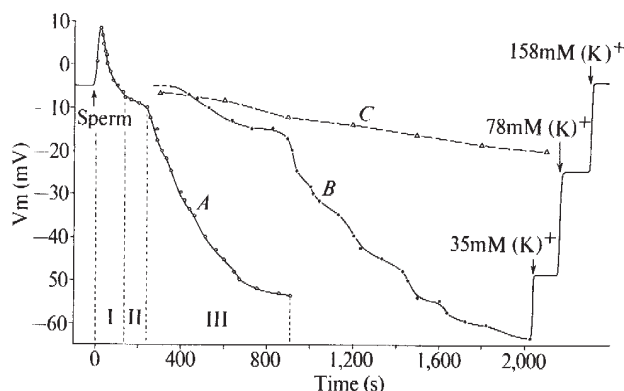


Fig. 1 A, Membrane potentials following normal fertilization. The phases I, II and III (ref. 1) are designated. B, Membrane potentials of an unfertilized egg exposed continuously to seawater adjusted to pH 9.1 with NH_4OH . After the potential had reached its final value, the external K^+ concentration was raised stepwise. The membrane potentials decreased as predicted for a K^+ concentration potential. C, Membrane potentials of an unfertilized egg exposed continuously to seawater adjusted to pH 9.1 with NaOH. Only the slow initial development is shown; the potentials ultimately reach -60 mV.

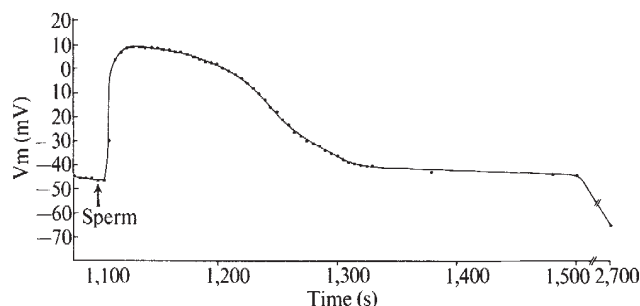


Fig. 2 Membrane potential changes upon insemination of an egg that had attained -45 mV after a 20 min exposure to NH_4OH seawater at pH 9.1. The depolarization and reversal of potential (action potential) indicate that the egg had remained unfertilized and capable of carrying out the phase I changes typical of normal fertilization.

ment of potential in seawater containing NaOH is taken to mean that the NaOH penetrates, but much more slowly. These inferences are justified by a long standing permeability principle which states that weak bases penetrate in their undissociated form⁴ and that the cell has little or no permeability to strong bases. The fact that the NH_4OH which penetrates increases the internal pH had been shown long ago in studies on sea urchin eggs, using pH indicators⁵⁻⁷.

The effect of the NH_4OH in invoking the phase III development of membrane potential is not an "activation" of the egg in the sense in which that term is used by students of fertilization and parthenogenesis. As we have shown¹, parthenogenetic activation includes a phase I action potential; none is seen in experiments with NH_4OH . There is no cortical reaction in NH_4OH ; no fertilization membrane is formed. (We note, however, Loeb's finding⁸ that sea urchin eggs will develop if treatment with ammonia is followed by an exposure to hypertonic seawater.) After the membrane potential has developed in NH_4OH seawater, the egg may still be fertilized with sperm and then go through the cortical reactions, followed by mitosis and cleavage.

When sperm were added to eggs that had begun or completed the phase III reactions in NH_4OH seawater, fertilization was accompanied by a phase I action potential observed as a very large depolarization (Fig. 2). The eggs then repolarized, somewhat slowly, to the -60 mV typical of the end of phase III.

To test whether the NH_4OH seawater only initiated the phase III development of membrane potential or whether it was also required to sustain it, eggs were acidified after the potential had begun to develop. The medium was brought to pH 6.8–6.9 by addition of seawater containing butyric acid (final concentration about 2 mM). The development of membrane potential stopped (Fig. 3). In some experiments, potential was lost before it levelled off. Simultaneously the membrane resistance increased. After some time in acid conditions, the medium was again brought to pH 9–9.1 with NH_4OH . The membrane potential now increased very rapidly to the value it would have had if the egg had been in the NH_4OH seawater continuously. The rate of increase was far greater than would be expected if the return to alkaline pH had merely restarted the development of the potential at its normal rate. We interpret this sudden increase in potential as a "readout" of a latent K^+ -conductance which had continued to develop in acid conditions.

We interpret these results as follows. (1) An increase in internal pH by NH_4OH initiates the development of K^+ -conductance in alkaline conditions; this development is "read out" as increasing membrane potential and decreasing membrane conductance. (2) If the pH is lowered during phase III, the development of K^+ -conductive sites in the membrane continues, but the channels are not open and the latent potential is not "read out". (3) With the return to alkaline conditions, the K^+ -channels are opened and the potential is read out immediately. The same dependence of "readout" on pH is observed with normally fertilized eggs in phase III. In fact,

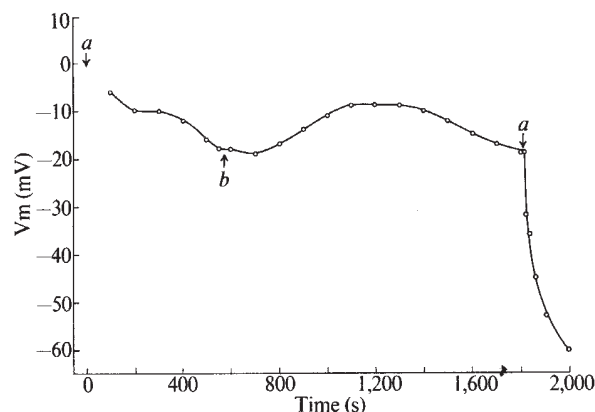


Fig. 3 Effect of pH on the development and "readout" or K^+ -conductance. The development of potential is initiated in NH_4OH seawater. Neutralization of the medium with butyric acid to pH 6.8 arrests the development of measured potential, with a temporary loss of potential. Restoration of pH 9 with NH_4OH results in a rapid "readout" of potential, indicating the continued development of latent K^+ -conductance during the period of exposure to acid. a, Basic seawater; b, acid seawater.

the progress of phase III in a fertilized egg is more rapid and more smooth in NH_4OH seawater than in normal seawater. So far we have not established whether or not the effect of pH on "readout" requires penetrating acids and bases.

In studies to be reported in detail elsewhere, we have asked whether the turning-on of the phase III membrane changes in the unfertilized egg by NH_4OH is accompanied by the release of other events that take place during the same period. From the time-table given by Epel *et al.*⁹ the activation of the egg is followed immediately by cortical changes and respiratory changes during the first minute or so. The next sequence of events, beginning around the fifth minute, is the activation of some membrane transport processes, the initiation of DNA synthesis and the acceleration of protein synthesis. The latter group of events would coincide with phase III. We can report that the development of K^+ -conductance in NH_4OH is accompanied by an increase of thymidine uptake, the initiation of DNA synthesis, the condensation of the chromosomes following DNA synthesis, and the breakdown of the nuclear membrane. No mitotic apparatus is formed. Protein synthesis is stimulated (experiments by D. Epel, in preparation). Thus, the presumed elevation of internal pH in an unfertilized egg that remains an unfertilized egg does release some important processes which normally follow after the activation of the egg by sperm.

We can imagine that among the first events of fertilization (for example, the respiratory burst and acid production) are some that change the intracellular pH, triggering the next group of events (the release of DNA and protein synthesis and the development of membrane properties). Our treatment with NH_4OH would be a replacement of mimicking of the normal link in the chain, starting those processes in an unfertilized egg that remains an unfertilized egg. The effect might be a common triggering of parallel events or might reflect a closer connexion between membrane potential and biosynthetic processes. Such connexions are being found in other systems¹⁰. The action of NH_4OH might in fact be an experimental substitute for a normal link in the chain, since it was reported long ago¹¹ that fertilization is accompanied by the production of ammonia.

This work was supported by grants from the US Public Health Service.

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Received August 14, 1972.

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Influence of Sodium, Potassium and Chloride Ions on the Intracellular Responses of Turtle Photoreceptors

VERTEBRATE photoreceptors have a low membrane potential (about 30 mV) in darkness and respond to illumination with a hyperpolarization associated with an increase of membrane resistance¹.

The effects of ions have been studied in retinas of frog and pigeon by recording extracellular responses attributed to the receptors. In both cases the size of the response was linearly related to the log of external sodium ion concentration². More recent work on the action of light on the sodium permeability of rod outer segments analysed in the isolated rat retina has shown that inward-directed current flows through the outer segment membrane in darkness and is reduced during illumination³. These observations suggest that vertebrate photoreceptors are highly permeable to sodium in darkness, and that light reduces sodium permeability and thereby increases the ratio of potassium to sodium permeability. If this interpretation is correct, membrane potential should be more sensitive to sodium concentration during darkness than during illumination and more sensitive to potassium concentration during illumination than in darkness. The experiments described here were performed to test these predictions.

The eye of the turtle, *Pseudemys scripta elegans*, was removed and cut along the medial-lateral axis. After draining the vitreous, the eyecup was mounted on a chamber where an oxygenated and buffered Ringer solution continuously flowed over the vitreous side of the retina at a rate of 5–6 ml./min. All solutions were freshly prepared; the pH was adjusted to 7.7 ± 0.1 with appropriate amounts of sodium bicarbonate or Tris chloride. Test solutions with low sodium concentration were made by replacing equimolar amounts of sodium chloride with choline chloride or Tris chloride or sucrose. Solutions containing potassium at high concentrations were prepared by replacing equivalent amounts of sodium chloride with potassium chloride. Solutions with low chloride concentration were prepared by replacing equimolar amounts of sodium chloride with sodium propionate or sodium sulphate. Intracellular recordings were made with glass microelectrodes, filled with 4 M potassium acetate. Responses were recorded differentially between two microelectrodes, one in the external solution and the other inside the cell. With microelectrodes of low tip potential⁴ there was no need to correct the results for changes of liquid junction potentials due to substitution of the bathing fluids. The retina was stimulated by white light from a tungsten quartz-iodine lamp run from a regulated d.c. supply providing 6.5 A. The reduced image of a circular

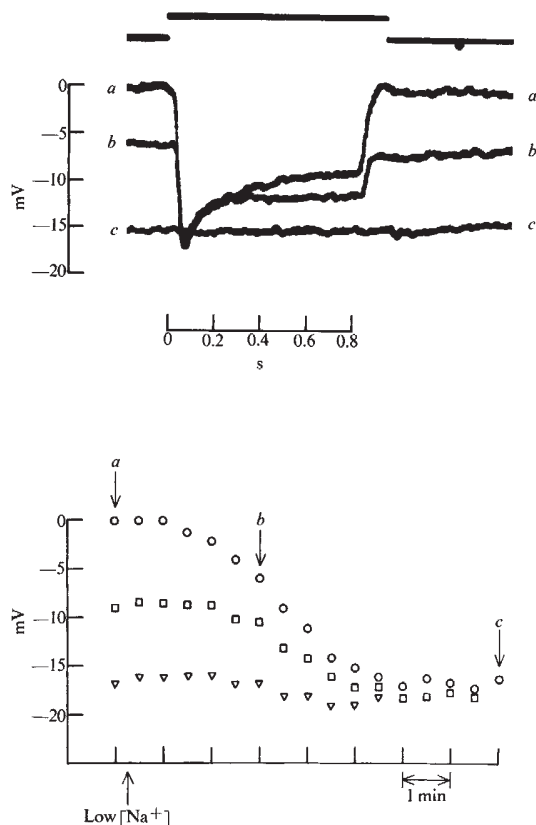


Fig. 1 Effects of low external sodium concentration on the intracellular responses of turtle cone. The records *a*, *b* and *c* on the top of the figure were taken at the times indicated respectively by the arrows *a*, *b* and *c* in the diagram below. At the time indicated by the upward directed arrow a sodium free solution made iso-osmolar with appropriate amounts of choline chloride starts substituting the control Ringer. The change of membrane potential is indicated relative to the transmembrane potential in darkness during the control period at the beginning of the experiment. The light intensity used to elicit the responses is 2.4 log units attenuated with respect to the total available energy. ○, Dark level; □, plateau of response; ▽, peak of response.

diaphragm was brought to focus on the retina where its diameter was 500 μm . The light intensity was attenuated by neutral density filters. The total irradiance of the unattenuated light of wavelength between 4,000 and 8,000 Angström was approximately $4.5 \times 10^3 \mu\text{W}/\text{cm}^2$.

The results of an experiment in which intracellular responses from a cone were recorded during perfusion of the retina with a sodium-free solution are shown in Fig. 1. The superimposed records (*a*), (*b*) and (*c*) at the top of the figure were taken at the times indicated by the arrows in the diagram at the bottom of the figure. In (*a*) light evokes the characteristic hyperpolarizing response in normal perfusing conditions; in (*b*) and (*c*) responses to light from the same cell were obtained at two different stages of sodium substitution. Clearly as the sodium-free solution exchanges with the normal Ringer, the membrane potential in darkness (circles) and the potential of the plateau of the response (squares) increase, while the potential at the peak of response is not appreciably altered. In all experiments the effects of low sodium concentration were reversed when the normal external concentration of sodium was restored.

The effect of high external potassium concentration was studied in separate experiments. A representative experiment is shown in Fig. 2. The records at the top were obtained in normal Ringer (*a*) and after exposure for several minutes to a solution containing 50 mM potassium (about twenty times the normal concentration) (*b*). The lower part of the figure is a plot of the membrane potential in

darkness (circles), during the plateau (squares) and at the peak (triangles) of the response to light.

These results show that the membrane potential of turtle cones is controlled predominantly by the sodium concentration in darkness and by the potassium concentration at the peak of the response. This is consistent with the idea that in darkness the membrane potential is due to the shunting effect of a high sodium conductance.

The plateau of the light response is appreciably modified by changing the external sodium concentration. This could be due either to an increase of sodium permeability following the initial reduction produced by light, or to a more complex effect reflected on the cone response by the feedback from the horizontal cells⁵.

The external potassium concentration affects consistently the membrane potential also in darkness. This suggests that the ratio of potassium to sodium permeability is not negligible even in darkness.

The hyperpolarizing effect of light can then be explained by assuming that sodium permeability is decreased as a consequence of illumination and the membrane potential changes approaching the level determined by the electromotive force of the potassium concentration difference. It seems likely that the light-dependent sodium permeability is located in the membrane of the outer segments, and that the inner segments are constantly permeable to potassium⁶.

I thank Drs M. G. F. Fuortes and E. F. MacNichol jun. for advice and criticism.

Note added in proof. While my manuscript was in the

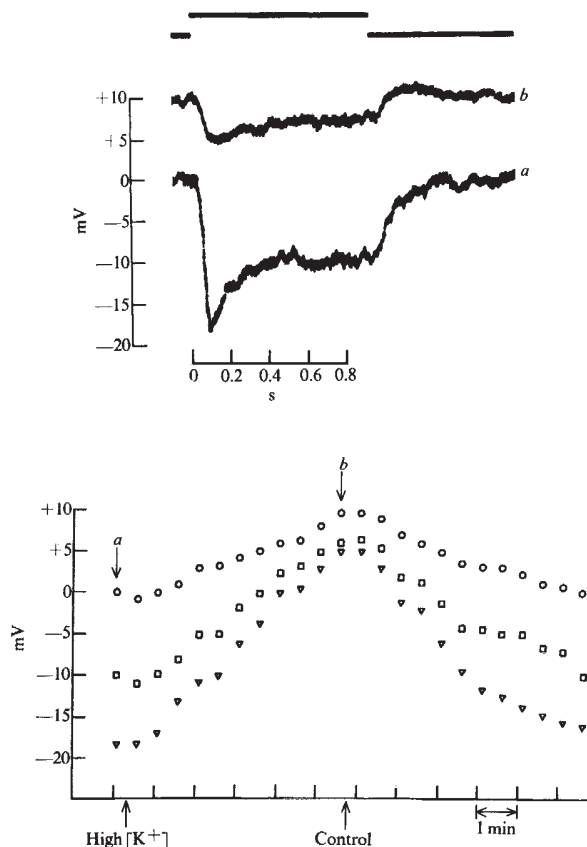


Fig. 2 Effects of high external potassium concentration on the intracellular responses of turtle cones. The records *a* and *b* at the top of the figure were taken at the times indicated respectively by the arrows in *a* and *b* in the diagram below. The zero level of membrane potential indicates the level of membrane potential in darkness. At the time indicated by the upward directed arrow a solution containing 50 mM of potassium starts substituting the control Ringer. The stimuli parameters are the same as reported in the legend of Fig. 1. ○, Dark level; □, plateau of response; ▽, peak of response.

press, similar results on *Bufo marinus* retina have been obtained by L. H. Pinto and J. E. Brown: see the abstract in *Biol. Bull.*, p. 473, October 1972.

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Correlation of Cell Shape and Function in the Visual Cortex of the Cat

FROM recordings made with microelectrodes it is known that neurones in the primary visual cortex (area 17) of the cat's brain respond to certain types of visual stimuli. An individual cell usually responds well to slits or edges of light shone at an appropriate orientation within a particular part of the visual field. Such a cell can be classified as simple, complex, or hypercomplex, in terms of the selectivity of its responses^{1,2}. Observations of Golgi-impregnated material have demonstrated that most cortical neurones are either stellate or pyramidal and it is natural to wonder whether the structural and functional properties of these cells are directly related. A suggestion that this may be so comes from the observation that the distribution of simple cells in different cortical layers tends to parallel the distribution of stellate cells, while a similar approximate correspondence exists between complex and pyramidal cells^{1,3,4}, which suggests this may be so. We have approached the problem of correlating cell structure and function more directly by injecting physiologically identified cells with a fluorescent dye, Procion yellow, using the technique developed by Stretton and Kravitz⁵. This dye spreads into even the finest processes of a cell and can give almost as clear a picture of cell morphology as a Golgi stain.

Cats were anaesthetized initially with ketamine hydrochloride, maintained in a state of light anaesthesia with intravenous thiopental sodium, and artificially respired while paralysed with succinylcholine. A 2 × 5 mm region of the primary visual cortex was then exposed, coated with 2.5% agar, and covered with a transparent saline-filled chamber that was sealed to the skull. The sliding top plate of this closed chamber contained a small aperture filled with 'Vaseline', through which a microelectrode could be advanced into any desired part of the exposed cortex. Glass microelectrodes were prepared by bevelling the tips with a grinding stone⁶ until the d.c. resistances were 20–80 MΩ and tip diameters 1–3 μm. The advantage of using bevelled electrodes is that their resistance is low enough to permit stable extracellular recording from cortical neurones, but they are sharp enough to penetrate cells.

In a typical electrode track through the cortex many single units were recorded extracellularly. The receptive field of each cell was mapped using spots and slits of light projected onto a tangent screen upon which the cat's eyes were focused. After

the receptive field characteristics had been determined with the electrode positioned extracellularly, an attempt was made to penetrate the cell by advancing the electrode slightly. If a cell was impaled, its receptive field was quickly re-mapped to be absolutely sure that the intracellular and extracellular recordings were from the same unit. Occasionally an intracellular recording was obtained that was sufficiently stable to make it possible to map the receptive field even though the responses of the cell had not previously been recorded extracellularly.

When a cell was satisfactorily classified, it was injected with Procion yellow by passing a steady hyperpolarizing current of 1–20 nA for 1–30 min. The electrode was then withdrawn and a new penetration was made at least 0.5 mm from any of the previous electrode tracks. Up to a dozen separate penetrations could be made in an experiment, and each entry point of an electrode was indicated on an enlarged photograph of the surface of the cortex using the blood vessels as landmarks. At the end of the experiment an electrode filled with a 4% Chicago blue solution was used to make several superficial dye marks on the cortex. These served as reference points for identifying individual electrode tracks during histological processing. The animal was perfused with 10% formalin and serial frozen sections (80 μm thick) were cut from area 17 and examined with fluorescence microscopy⁵.

In view of the numerous technical difficulties, it was not surprising that only one or, at most, a few cells were stained in each experiment. We have injected a total of sixteen physiologically identified neurones so far, and to illustrate our results we shall describe in detail two cells having particularly well-defined properties. The first of these cells responded optimally to a narrow slit of light moved slowly across its receptive field in a 2 : 00–8 : 00 orientation (Fig. 1A). This unit was clearly a simple cell, as defined by Hubel and Wiesel¹, because, on the basis of the responses to stationary spots and slits, its receptive field could be subdivided into a central 'off' (inhibitory) zone flanked by antagonistic 'on' (excitatory) regions. During subsequent histological examination of the cortex, a single fluorescent neurone was seen in the region where this unit was injected. The cell was located 350 μm below the pial surface, in layer IV a + b of Otsuka and Hassler⁴ as determined from the cyto-architecture of an adjacent Nissl-stained section. A camera lucida drawing of this neurone shows a cell body about 15 μm in diameter and somewhat irregular in shape (Fig. 1B). Most of the processes radiating from the soma seem to be dendrites, because they divide into finer branches that are beaded and show occasional dendritic spines. One descending process, labelled 'a', is more uniform in diameter, has no spines and is probably the axon. This neurone has the characteristic features of stellate cells seen in Golgi preparations of the visual cortex³.

Fig. 2 illustrates a pyramidal cell injected in a separate experiment. The optimal stimulus for this unit was a vertical slit moved from right to left across its receptive field (Fig. 2A). Stationary vertical slits produced brisk discharges at 'on' and 'off' when positioned anywhere within the receptive field, but a rectangle filling the field gave only a weak response, indicating that this was not a simple cell. Nor was the cell hypercomplex, since the response to a slit was not diminished when the stimulus was lengthened so as to extend above and below the receptive field. It thus fulfilled the criteria for a complex unit¹. A partial photographic montage and a camera lucida reconstruction of this cell are shown in Fig. 2B and C. The pyramidal-shaped cell body is situated in layer V, 700 μm below the pial surface. A long apical dendrite radiates upward at least as far as layer III, and several basal dendrites and horizontal processes extend from the cell body. A descending process that is probably the axon can also be seen.

In some of the cells that were injected, either the functional or the morphological properties of the unit were not as clear-cut as in the two examples just described. For example, it was often difficult to map the receptive field of a unit, either because

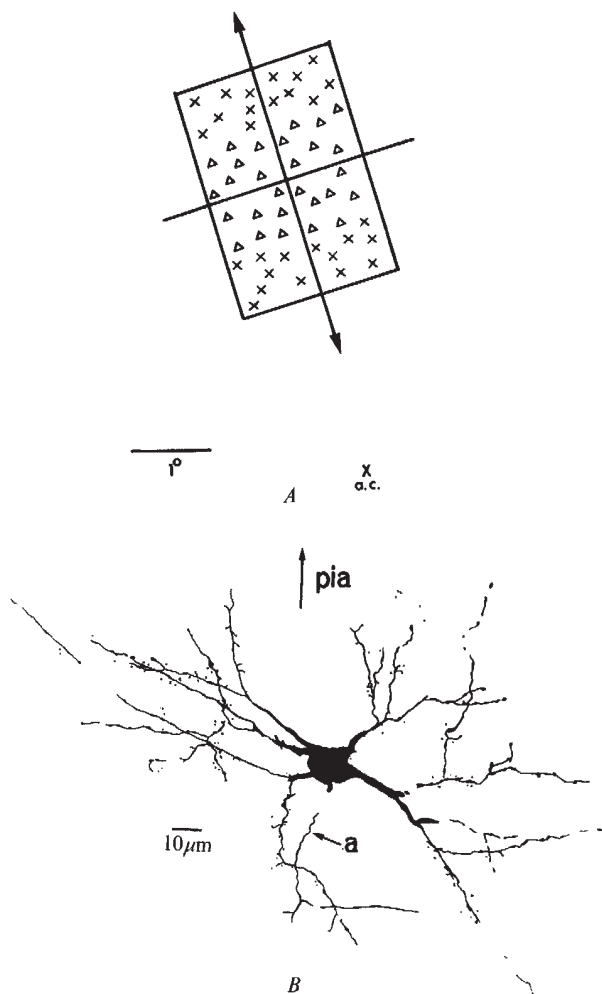


Fig. 1 *A*, Receptive field map of a simple cell that was subsequently injected with Procion yellow. This cell was driven by stimuli to the left eye; it responded best to a narrow slit of light held in a 2 : 00–8 : 00 orientation and moved in either direction orthogonal to the slit (arrows). Stationary spots and slits of light also drove this unit. X, Areas giving “on” responses; triangles, areas giving “off” responses. The cell showed only a 5 mV resting potential after penetration, but it was successfully stained by passing a steady 10 nA current for 20 min. The large X indicates the projection on the visual field of the area centralis (a.c.) of the left eye. *B*, Camera lucida drawing of the same cell, which lay in two adjacent coronal sections of the cortex. Drawings were made using bright-field fluorescence illumination and a $\times 100$ oil immersion objective which allowed processes less than 1 μ m in diameter to be visualized. Most of the branches emerging from the cell body show occasional faint appendages that are likely to be dendritic spines. One descending process “a” is more uniform and is probably the axon.

its responses were weak or because they were partially obscured by injury discharges produced by the electrode penetration. Further, the structural features of a neurone were sometimes intermediate between those of well defined stellate and pyramidal types, presumably because the cell was not completely stained. Many injected cells had to be discounted altogether for such

Table 1 Summary of Identified Cells		
	Definite	Probable
Simple cells: stellate	4	1
pyramidal	0	1
Complex cells: stellate	1	0
pyramidal	4	3
Hypercomplex cells: stellate	1	0
pyramidal	1	0

reasons. Of the remaining sixteen neurones, nine were identified with a high degree of confidence, while the classification of the other seven was probable but not definite.

Most of the simple and complex cells that were injected conformed to a general pattern. Of the five clearly identified simple units, four were definitely stellate cells and the fifth was probably stellate also; one probable simple cell was definitely a pyramid. Five injected cells had well defined complex receptive fields; four of these cells were pyramidal and one was stellate. The three probable complex units were all pyramidal cells. We have also stained two hypercomplex cells, one of which was pyramidal and the other stellate. Both of these hypercomplex cells, as well as the two exceptions to the general pattern seen for simple and complex cells, were found in cortical layers II and III, suggesting that not only

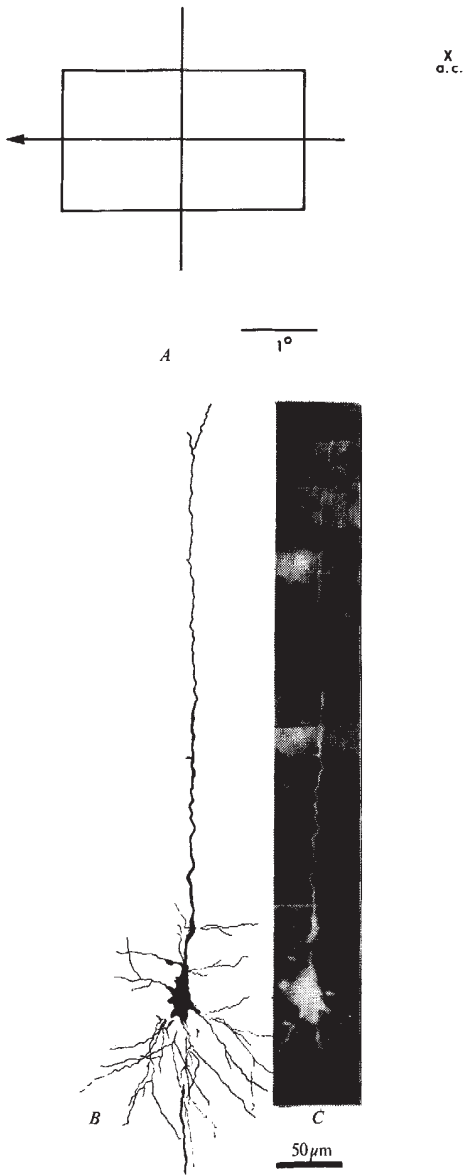


Fig. 2 *A*, Receptive field of a complex cell that responded optimally to a vertical slit. The cell was driven equally well by both eyes and had a moderate directional preference (right to left). Stationary vertical slits produced discharges at “on” and “off”, but stimuli more than 30 degrees from vertical gave little or no response. The cell was penetrated without previous extracellular identification; the resting potential was 40 mV. To inject the cell, a 1–2 nA hyperpolarizing current was passed for 15 min. *B*, Camera lucida drawing of the same cell. The cell is similar in appearance to the large pyramidal cells seen in Golgi preparations. *C*, Partial photographic montage of this cell, which was a large pyramid situated in layer V.

cell shape but also location within the various layers of the cortex are functionally important.

These experiments demonstrate that it is possible to inject dye into individual stellate and pyramidal cells in the mammalian cortex. Although our electrodes may be biased towards selectively impaling relatively large cortical cells, we have been able to inject and recover neurones whose somata are less than 10 μm in diameter. Our results, while still preliminary, support the idea that the morphological and physiological properties of cortical neurones are closely related. Most injected simple cells were stellate, and most complex cells were pyramidal; we have not yet stained any of the cortical cells that are neither stellate nor pyramidal, such as fusiform or multiform cells. A larger sample of cells is obviously needed to determine more precisely the relationship between structure and function in each cortical layer. Our hope is that this approach will eventually lead to a more complete understanding of the different stages of information processing within the cortex.

We thank Drs David Hubel and Torsten Wiesel for their aid during these experiments. This project was supported by the US National Institutes of Health.

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Received October 6, 1972.

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Thorne Moor: a Palaeo-ecological Study of a Bronze Age Site

A STUDY of the palaeo-ecology of an area in South Yorkshire has revealed an exotic element in the Bronze Age insect fauna (about 1100 BC), together with evidence for southerly restriction in the distribution of some British species. This fauna has been studied in relation to a number of archaeological features relating to forest clearance.

Thorne Moor (National Grid ref. SE 730160), a fast shrinking relict of a once extensive blanket bog, lies 15 km north-east of Doncaster. The area is largely below the 10 foot OD contour and is floored by lacustrine deposits of the last glaciation, which form a gently undulating topography originally covered by up to 2.0 m of peat (now largely removed for commercial exploitation).

In October 1971, Mr W. Bunting drew our attention to a number of large, superficially burned tree trunks being exposed by the cutting of drainage ditches. Subsequent investigation demonstrated two limited areas, one about 50 m across, wherein most of the large trees, which lie at the base of the peat, had charred surfaces. Much of the fossil timber, all from the lower 0.5 m of the peat, was riddled with insect galleries and a study both of this and of the associated organic muds and peat was initiated. During sampling, a number of parallel split timbers in a fresh ditch section were observed (J. R. A. Greig, personal communication). On excavation this was found to be part of a trackway of simple construction in the peat at the same level as many of the trees. This and the areas of burnt trees have been interpreted as representing a phase of small temporary clearances in the mixed oak forest, preceding the main period of peat formation on the Moor. The carbon-14 age of 3090 ± 90 BP (Birm. 336) gives a Bronze

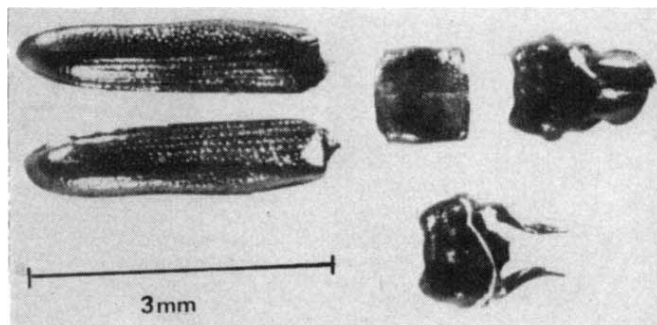


Fig. 1 *Prostomis mandibularis* (F).

Age date for these activities and agrees with the earliest clearance horizons defined on palynological grounds¹. Dr M. Herbert-Smith has found significant quantities of cereal pollen between the timbers of the trackway. The insect fauna and plant macrofossils of this period indicate phases of open water, perhaps in the form of confluent pools, over much of the area, resulting in the decline and death of the forest trees. This process was sufficiently rapid for the deposits immediately above the forest soil, around the roots of trees which were still standing, to contain a mixture of many water beetles and insects associated with the moribund forest. The construction of the trackway was a response to the increasingly wet conditions, the waterlogging finally curtailing all agricultural activity in the area. Despite the complete absence of supporting archaeological data, the decrease in arboreal pollen during the Iron Age was interpreted as marking the onset of more intensive agricultural activity¹; the end of the forest on Thorne Moor may now be ascribed to a cumulative and widespread process of flooding. The Moor lies well within the tidal reach of the Humber and, like the early post-Roman flooding at York², the mechanism may be changes in the configuration of the mouth of the Humber—the destruction by the sea of an earlier Spurn Point—increasing the access of the tides to the estuary, thereby backing up the outflow of the rivers and causing freshwater flooding.

The insect fauna includes non-British and Southern English beetles. Many well-preserved specimens of *Prostomis mandibularis* (F.) (Cucujidae) (Fig. 1) have been recovered, both from beneath bark and from associated organic silts, the greatest numbers coming from between the timbers of the trackway. All the examples from Thorne were associated with oak. The species, previously unknown in Britain, is recorded as living in the extreme south of Sweden, throughout Europe south of the Baltic, in the USSR and North America. It is regarded as a species of ancient forests and lives in wood which is in an advanced state of decay³. A single thorax of *Rhyodes sulcatus* F. (Rhyodidae) was found in an accumulation of silt in a hollow between the roots of a large oak stump. *R. sulcatus* now has a southerly distribution in Europe and is also known from the Caucasus and Anatolia⁴. It has been previously recorded from a deposit of Neolithic age (¹⁴C age, 4830 ± 100 (NPL 39)) at Shustoke in Warwickshire⁵. It is found in rotten wood and is probably a predator. *Zimonia grossa* (L.) (Ostomidae) (Fig. 2) has a wide boreo-montane distribution in continental Europe and across the USSR to Baikal. The larvae bore in rotten wood, pupation occurring in a dilation of the tunnel just beneath the bark. A single complete male individual was recovered from such a chamber below the bark of a birch, which lay in the upper part of the basal peat, probably dating from the Early Iron Age. It is worth noting that *Z. grossa* is recorded from old woodland which has been damaged by forest fires³. *Mycetina cruciata* Schall. (Endomychidae) shows a similar European distribution to *Z. grossa*⁶. It is locally common beneath the bark of both deciduous and coniferous trees, associated with *Polyporus* fungi. A single specimen was found between the wet, decayed timbers of the trackway. The Eucnemid, *Isorhipis melasoides*

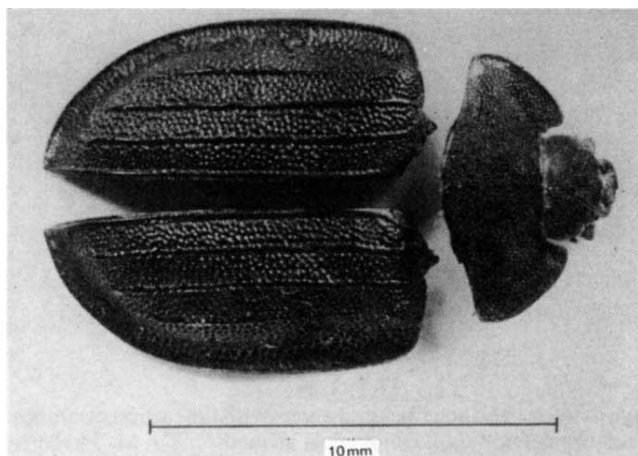


Fig. 2 *Zimioma grossa* (L.) ♂.

(Cast.), was at least occasionally abundant at Thorne; its characteristic workings were frequently noted and a number of complete adults and many larval head capsules were found by splitting timber. The species, normally associated with beech⁷, is now rare on the Continent and is restricted to areas away from the western seaboard and the Mediterranean.

Further to this list from Thorne, it is relevant to include the specimen of *Cerambyx cerdo* L., found in a bog oak from the Cambridgeshire Fens⁸. This specimen has been re-examined and found to be contemporary with the wood, dated to 4000 ± 66 BP (Birm. 1). In agreement with Allen⁹ and Morley¹⁰, this species can now be finally removed from the list of insects which have lived in Britain since systematic recording began, probably having disappeared well before the advent of the early entomologists who erroneously recorded it.

A number of species found in the Thorne samples do not now live as far north as Yorkshire and have obviously restricted their ranges since the Late Bronze Age. These include *Colydium elongatum* (F.), *Dryophthorus corticalis* (Payk.), *Hypulus quercinus* (Quens.), *Mesosa nebulosa* (F.), *Microlophus parallelopipedus* (Hbst.), and *Platypus cylindricus* (F.). All are associated with wood. In addition, Skidmore has recorded *Gnorimus nobilis* (L.) from a deposit of similar age at Askern, 10 km north of Doncaster, well north of its present distribution¹¹.

The disappearance from Britain of the five species from Thorne, of *C. cerdo*, and of the dung beetle *Aphodius quadriguttatus* (Hbst.) and *Dermestes lanarius* Ill., a relative of the common bacon beetles, recorded in the fauna from the Wilsford Shaft, Wiltshire (3330 ± 90 BP (NPL 74))¹², and the restriction in range of many others depends on a number of factors. The hypothesis of climatic changes towards a more oceanic regime is supported by the present European distributions of these species, which in general exclude areas of damp mild climate. This correlation may not, however, be justified for, as Osborne has suggested¹³, the anthropogenic factor must always be considered once man has started to break the forest cover. The Thorne fauna is contemporary with such an event and, although the total disappearance of dense woodland in this locality is related to geomorphological factors, elsewhere it is evident that, as clearance proceeded, habitats became both restricted and separated. Many species dependent on mature forest must have been reduced to populations whose small size would render them liable to extinction by any minor environmental change, the isolation of each community precluding re-immigration. The German word *Urwaldtiere*—old forest animals—would seem most appropriate for such insects¹⁴. A good example is provided by *Teredus cylindricus* (Ol.) (Colydiidae). The beetle, abundant in the Thorne trackway samples, is now rare, restricted to parts of Sherwood Forest and Windsor Forest, poor substitutes

for the ancient forest environment. This species and many others associated with rotten wood habitats must be regarded as endangered animals, not only in Britain but throughout Europe.

This note owes much to the work and stimulating discussion of P. J. Osborne and to W. Bunting's untiring efforts to protect what is left of Thorne Moor. Professor F. W. Shotton kindly read and commented upon the manuscript. This work was carried out during the tenure of NERC grants.

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Ultraviolet Differences between the Sulphur Butterflies, *Colias eurytheme* and *C. philodice*, and a Possible Isolating Mechanism

Two common North American "sulphur" butterflies, *Colias eurytheme* and *C. philodice*, hybridize widely over much of the United States. The planting of dense agricultural stands of larval foodplants, together with habitat destruction starting about 1850, are believed to have affected the species' geographical ranges and abundances, causing extensive sympatry over much of the eastern United States¹, setting the stage for introgression. Taxonomists have tried to separate the species on the basis of visible wing coloration, but this varies continuously from orange (pure *eurytheme*) through yellow-orange (*eurytheme* and many hybrids) to yellow (*philodice* and rarely *eurytheme*). Female polymorphism and numerous described infraspecific taxa compound the taxonomic confusion. Although some of this variation has been attributed to genetic (refs. 2 (and references therein) and 3) and environmental factors⁴, species determinations are often guesswork. We wish to point out two correlated qualitative characters that can be used to separate these species. One character, ultraviolet reflexion, may be more than a taxonomic tool—it may be the butterflies' own *carte de visite*.

Males of *eurytheme* have a brilliant ultraviolet reflexion pattern (Fig. 1), easily visualized by ultraviolet video-viewing⁵ or photography. The iridescent coloration, of structural origin, is produced by interference in layered lamellae that make up the ribs of special wing scales (R. Silberglied, thesis, Cornell University, 1969; Figs. 2b and 3b of ref. 6 vaguely show the

lamellar structure, although the authors do not comment on it). Superficially identical structures occur in wing scales of *Eurema lisa* (Pieridae)⁷. These scales, and the ultraviolet pattern they produce, are absent in males of *philodice* and in all females of both species. Although the lamellar structure of the ribs cannot be resolved with the light microscope, visible correlates can. The distance between the parallel ribs of ultraviolet-reflecting scales is considerably less than that of other scale types (Fig. 2). Their lack of distal lobing^{8,9} is often distinctive. The ultraviolet reflexion pattern of male

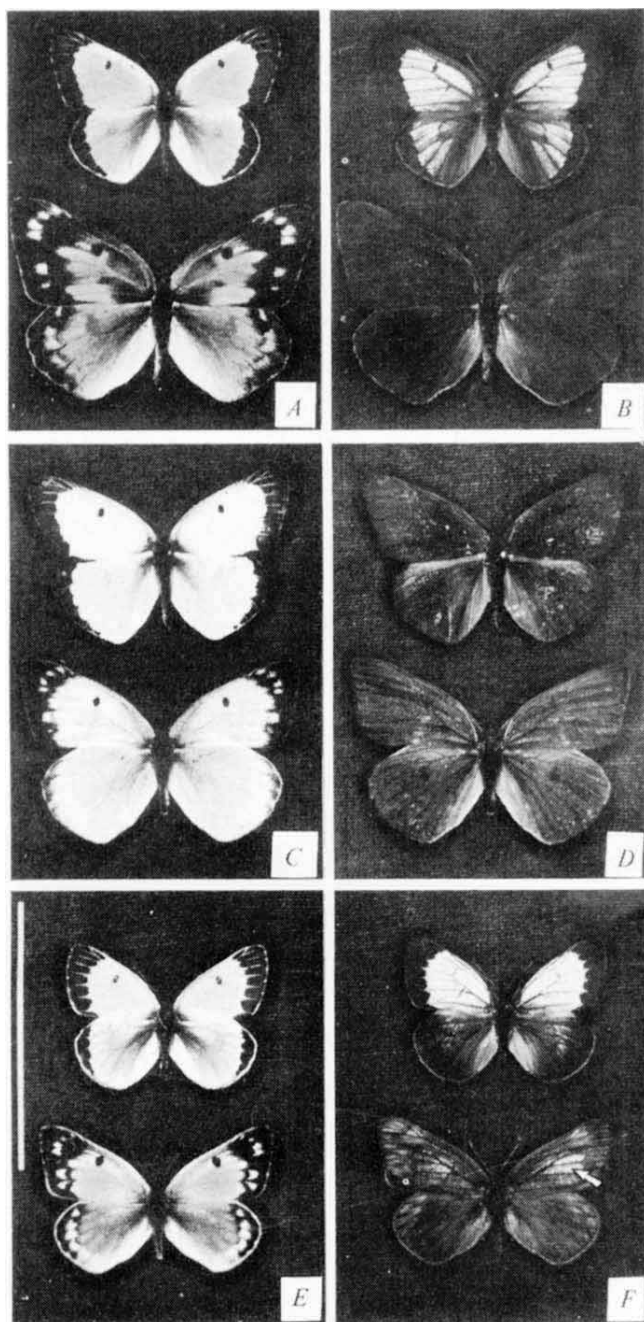


Fig. 1 *Colias eurytheme* (visible coloration orange; A, B), *C. philodice* (yellow; C, D), and *C. chrysotheme* (orange; E, F) in visible (400–700 nm, left) and near ultraviolet (300–400 nm, right) light. The male is uppermost in each photograph. Arrow in F indicates UV-reflecting patch of female *C. chrysotheme*. Reference line for all figures (in E), 5 cm. (A, B: male, Elsinore Lake, California; September 1, 1917; female, Columbus, Georgia, July 23, 1944; C, D: male, West Newton, Massachusetts, September 6, 1900; female, Tyngsboro, Massachusetts, July 22, 1914; E, F: male, Leitha Gebirge Mountains, Austria, 1928; female, vicinity of Vienna, Austria, 1936.)

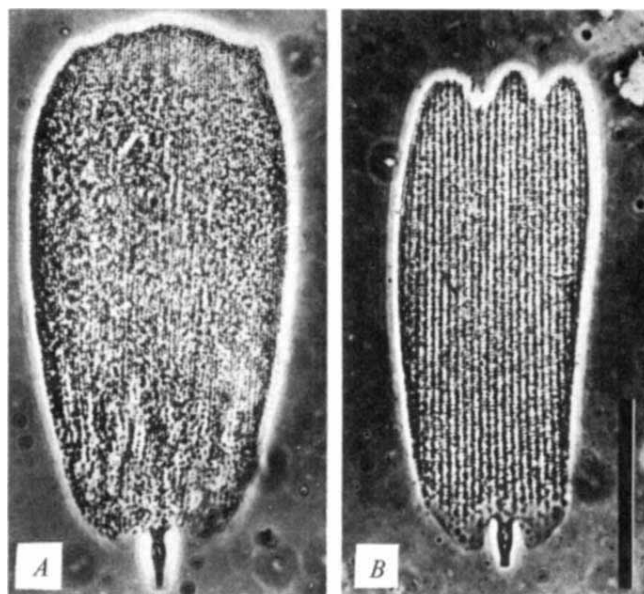


Fig. 2 Uppermost (“cover”) wing scale of male *Colias eurytheme* (A), characterized by ribs about or less than 1 μ m apart, reflects ultraviolet light; that of *C. philodice* (B), with a wider rib spacing, does not. Apical lobing of the scale may not be diagnostic for lack of reflexion. The lamellar structure of the ribs (in A), responsible for ultraviolet reflexion, cannot be resolved with the light microscope. Reference line for both figures (in B), 50 microns; polarized phase illumination.

eurytheme usually corresponds with the distribution of visible yellow or orange coloration, but the veins, which lack these scales, are dark. Correlation of scale structure with ultraviolet reflexion has also been reported in *Gonepteryx* (refs. 10–16 and those in ref. 12), and *Pieris*¹⁷.

Ultraviolet reflexion is a qualitative character. Examination of natural and laboratory-reared males of both species and interspecific hybrids has revealed only two phenotypes: ultraviolet-reflecting and ultraviolet-absorbing. This contrasts with the highly variable, pigment-produced, visible colours of *Colias*.

More than 12,000 male specimens, representing all described geographical units of *eurytheme* and *philodice*, were examined. All (>1,000) male specimens from allopatric *eurytheme* populations are ultraviolet-reflecting, including seven yellow specimens from the southern United States and Mexico. All (>500) allopatric male *philodice* (including “eriphyle” and “kootenai”) are ultraviolet-absorbing. (Males from the isolated Guatemalan population of “*philodice*” are ultraviolet-reflecting but have a pattern different from *eurytheme*, and represent a population of a different species complex.) Most field-collected yellow-orange specimens from areas of sympatry, suspected of having an F₁ or other hybrid origin, are ultraviolet-absorbing, but a small proportion are ultraviolet-reflecting. All (>10,000) females examined are ultraviolet-absorbing. White (“alba”) females reflect slightly more than “normal” females, but are never iridescent. Specimens of *eurytheme* collected early or late in the flight season, charac-

Table 1 Ultraviolet Phenotypes of Gerould’s¹⁸ Two Largest F₂ Interspecific Crosses, Demonstrating Sex-linked Inheritance of Ultraviolet Reflexion in *C. eurytheme* and *C. philodice*

Gerould’s cross number	1913q	1914g
P ₁ ♂	<i>Philodice</i>	<i>Eurytheme</i>
P ₁ ♀	<i>Eurytheme</i>	<i>Philodice</i>
Expected F ₂ ratios (males)		
If sex-limited only	3 UV– : 1 UV+	3 UV– : 1 UV+
If sex-linked	All UV–	1 UV– : 1 UV+
Observed (males)	13 UV–	8 UV– : 7 UV+

UV–, ultraviolet-absorbing; UV+, ultraviolet-reflecting.

terized by a localization of orange colour, have the same ultraviolet reflexion patterns as those collected in midsummer. Even the rare all-black males have the characteristic ultraviolet reflexion patterns.

All 155 F_1 male progeny of thirty-two interspecific crosses, involving both male *eurytheme* $\times$ female *philodice*, and male *philodice* $\times$ female *eurytheme*, were ultraviolet-absorbing. Examination of Gerould's classic F_2 broods¹⁸ at Yale University¹⁹ (Table 1), as well as numerous recent experimental crosses (by O. R. Taylor), revealed that the ultraviolet-reflecting character is inherited as a sex-linked recessive. The genetics of interspecific ultraviolet pattern inheritance will be treated in detail elsewhere (R. E. S., O. R. T. and S. A. Ae, in preparation).

Allopatric populations of *eurytheme* are homozygous for the (ultraviolet-reflecting) recessive allele (*uu*) and allopatric *philodice* populations are homozygous for the (ultraviolet-absorbing) dominant allele (*UU*). Sympatric populations in Arizona where interspecific mating is frequent, as well as laboratory crosses, contain yellow (*philodice*-like) but ultraviolet-reflecting, and orange (*eurytheme*-like) but ultraviolet-absorbing individuals, indicating that the ultraviolet alleles segregate from colour alleles and introgress into both parental populations.

Interspecific mating between these two species is not random²⁰, as had been reported^{21,22}. In the American mid-west and Arizona, comparisons of mating pair samples ($N=6,032$) from sympatric populations with random samples of adults show that mating is always positively assortative and differs significantly from random. Interspecific mating increases with density. The explanation lies with female behaviour during maturation. Mating obtained in the field with females of varying ages has shown that interspecific mating occurs almost exclusively when males can find and mate with freshly-emerged females (<1 h old) that are too young to reject them—a common phenomenon in dense populations in Arizona. Female mating preferences were determined by: (1) observing the courtships and matings ($N>3,000$) of 100–150 virgin females of each phenotype in mixed populations; (2) comparing the expected and observed frequencies of the different mating combinations in large samples of field-collected mating pairs; and (3) obtaining matings in a mixed population in a large outdoor cage (O. R. T., thesis, University of Connecticut, 1970).

Virgin females of *eurytheme* reject the courtship attempts of F_1 hybrid, ultraviolet-absorbing (*Uu*) males, while females of *philodice*, whose natural mates are also ultraviolet-absorbing, accept them (O. R. T., in preparation). Sampling from an area of recent sympatry in Arizona shows that males possessing unusual colour and ultraviolet combinations are at a disadvantage in mating. Selection seems to act against ultraviolet-reflecting hybrids (that is, F_2 , or $F_1 \times eurytheme$) through the mate-selecting preferences of the females. In an Arizona population significantly fewer ultraviolet-reflecting hybrid males were found among mating pairs (1.2%/662) than expected based on the frequency of these males in random samples (5.7%/653; $\chi^2=25.14$, $P<0.001$). Such evidence supports the hypothesis that the ultraviolet character functions as an isolating mechanism, or is highly correlated with an isolating mechanism that is unspecified at present. The ultraviolet character, however, is not the only isolating mechanism involved, since one of us²³ has shown that olfaction plays a major role in mate-selection by females. The latter result suggests that pheromones produced by the male are also used during courtship.

Visual stimuli release courtship behaviour in most butterflies, including *Colias*. Sexual dimorphism of ultraviolet reflexion is common throughout most of the Pieridae^{10–12} and those species tested behaviourally^{24–26} and electrophysiologically²⁷ (*C. eurytheme*) are sensitive to ultraviolet light. In Japanese *Pieris rapae*, ultraviolet reflexion of female wings releases male courtship behaviour²⁵. *C. eurytheme* males approach females much more frequently than they approach

other males, indicating that males may use the ultraviolet pattern to distinguish between the sexes. These facts, together with evidence that mating behaviour in pierid butterflies is affected by light intensity and spectral quality²⁵, suggest that ultraviolet patterns function in mate-selection in these species.

Finally, our species of *Colias* are not synonymous with the palaearctic *C. chrysotheme*, as some previous authorities have contended^{28,29}. Unlike both *eurytheme* and *philodice*, both sexes of *chrysotheme* have iridescent ultraviolet reflexion (Fig. 1). Other problematic species and species groups in the genus can be readily characterized by means of ultraviolet reflexion patterns (R. E. S., in preparation). It has been stated that *Colias* could not be monographed because of "variation and hybridization problems"³⁰, and that "no other genus of butterflies . . . presents so many baffling and stimulating problems"³¹. Some of the answers, it now seems, have been elusive simply because they lie beyond our visible spectrum.

We thank T. Eisner, A. M. Shapiro and E. O. Wilson for advice; L. L. Pechuman, F. H. Rindge, J. M. Burns, C. L. Remington, J. R. Heitzman and K. Roever for permission to examine preserved material; J. Sorenson for help in the field; and F. DeCosta and R. Desmaisons for loans of equipment. This study was supported in part by grants from the National Science Foundation to the University of Connecticut (O. R. T.), the General Research Fund of the University of Kansas (O. R. T.) and Harvard University (R. E. S.).

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BOOK REVIEWS

Violence in Perspective

The Challenge of Violence. Edited by Derek Richter. Pp. 174. (Ardua: Tadworth, Surrey, 1972.) £1.90.

Is Peace Inevitable? Aggression, Evolution and Human Destiny. By Santiago Genoves. Pp. 194. (George Allen and Unwin: London, August 1972.) £2.50.

It is seldom that a brace of books arrives on one's desk of which, having read, one can speak with unrestrained enthusiasm. Both deal with violence and hostility in the world of man, and each makes a genuine contribution toward the clarification of the issues, and at the same time offers realistically practical suggestions directed toward the solution of these problems.

The Challenge of Violence is published under the auspices of Amnesty International, which will receive proceeds from the sale of the book. Let me say at once that I have never read a volume by different contributors in which each of their essays is of such sterling quality, and which together so beautifully intercorrelate and reinforce one another. Usually such volumes are something less than exciting. This one I read, as Shelley used to say, in a frenzy of enthusiasm. *The Challenge of Violence* is the best symposium of its kind I have ever read. Dr Derek Richter's introductory essay, "Psychiatric Aspects of Violence", is certainly one of the best balanced analyses of the nature of violence with which I am familiar. Dr Richter sees violence as a form of insanity, and faulty upbringing at the heart of the problem. Education in the ability to think and to feel as an interdependent humane being, principally achieved through reconstituted schools, is impressively discussed by Geraldine Lack in "The Contribution of the Schools". Alan Bestic examines the prevailing violence in Northern Ireland, in an essay curiously mistitled "Inter-Racial Violence". But it is nonetheless illuminating, for all that. William Burrows, psychiatrist, writes with wisdom and insight on "Students in Revolt" on American college campuses, and David MacSweeney helpfully discusses "The Concept of Maturity", a stimulating analysis

for a world in which too many people grow older without growing up. But should one really ever grow up? MacSweeney himself speaks of Bertrand Russell, "the most brilliant child who ever lived to be nearly a hundred". Paedomorphosis is a very good idea. In "Women Working For Peace" Frances Eliot rightly emphasizes the importance of working within an organization, and in a moving essay, "Prisoners of Conscience", Eric Baker shows how successful organizational efforts can be in the rescue of political prisoners from governments who often do not know how large their number is. Is it not time that the unquestionable authority of the state were questioned? And what is the state if not an abstraction consisting of men? And is it not men, rather than abstract entities called states, who commit crimes against humanity? And should they not be held responsible for their crimes against humanity, such men? And was not that the principal injunction of the Nuremberg Tribunal?

In "The International Arms Race" Robin Clarke offers a thoroughgoing anatomy of the contribution of scientists and technologists to the progress of nuclear suicide, and the somehow inviolable notion that science must go on. "If such high proportions of our science and technology are used for military ends, it would obviously be a safer world if there were less science and technology." Finally, Sir Geoffrey Vickers in "The Containment of Conflict" gives us a really brilliant exposition of the conditions necessary for the achievement of a stable peace between modern societies. It forms a quite admirable pendant to this jewel of a book.

The Challenge of Violence is a book which should be read by everyone who considers this world worth saving. I hope the book is read in schools and universities. It would make a fine whetstone upon which to sharpen one's wits in study groups, seminars, and in similar discussion groups.

Is Peace Inevitable? by the Mexican anthropologist Santiago Genoves, is subtitled "Aggression, Evolution, and Human Destiny". It is a splendidly

sound and clarifying volume. Genoves is one of those physical anthropologists who fully understands that a knowledge of man's social evolution constitutes an indispensable dimension of the physical anthropologist's equipment. He writes with impressive authority from the holistic viewpoint of man's biosocial evolution. What Genoves is principally concerned with is the critical examination of current popular theories concerning man's nature and the nature of war. The theories of Lorenz and Ardrey come in for particular treatment, and are disposed of in masterly fashion. The fuzzy concept of "aggression" is carefully dissected, and the nature of aggression among animals clearly explained. In *On The Origin of Species By Means of Natural Selection, Or The Preservation of Favoured Races in the Struggle For Life* (the full and significant title is seldom given), Darwin, in using the phrase "the warfare of nature", confirmed and perpetuated the prevalent view. The idea more than lingers on. Some day, when the social history of biological thought comes to be written, future readers will be astonished to learn how much of biological theory was influenced by sociopolitical thought.

It is not an accident that the rise and popularity of the Lorenz-Ardrey interpretations of man's "aggressive nature" should be so appealing in this age of violence. The new social Darwinism is paying high dividends because, among other things, it is so consoling to know that violence is "in the nature of things", and especially in the nature of man. The burden of guilt is relieved, and one may in good conscience acquiesce in every form of sanctioned violence with the sort of rationalized disengagement that contributes so massively to the perpetuation of state violence and the increase of individual violence. It is the state of mind of the majority of Americans who believe that what their government has been doing in Vietnam is in the interest of peace. Violence, of course, is not violence when it is used to repel violence. And so the debasement of thought and language goes on, and *Nineteen Eighty Four* is well on its way.

It is good to have such a book as Dr Genovés's at this time, for while he is a master at correcting epidemic errors, he is also full of hope. With the authors of *The Challenge of Violence* he independently looks for our salvation in the direction of education for humanity, in the necessity of love. *Is Peace Inevitable?* is a book of outstanding merit.

ASHLEY MONTAGU

Scanning Monographs

The Scanning Electron Microscope. By C. W. Oatley. Part 1. *The Instrument.* Pp. viii+194. (Cambridge University: London, September 1972.) £5.30; \$16.50.

The Use of the Scanning Electron Microscope. By J. W. S. Hearle, J. T. Sparrow and P. M. Cross. Pp. xi+278. (Pergamon: Oxford and New York, September 1972.) £8.80.

THE scanning electron microscope is outstanding among modern scientific instruments, a type case in the technoeconomics of innovation and at the same time spectacular in its results. In the form now generally available, and used for the study of surfaces, it is of entirely academic provenance, having been devised and developed in the Engineering Department of the University of Cambridge under the guidance of the author of one of these books, C. W. Oatley. To an even greater extent than the conventional transmission electron microscope, which came out of the Technische Hochschule of Berlin, the scanning microscope was born by technological expertise out of scientific curiosity. Ten years in gestation and upbringing, it was gradually refined in efficiency of operation and tested on such applications as were ready to hand. Only then was it adopted by an instrument making firm next door, and fully developed as a research tool. Its success in the market has been remarkable as the value of scanning microscopy for surface investigation came to be appreciated. Because the displayed image is responsive to the electrical, magnetic and in some degree also the chemical state of a surface, as well as to its topography, the instrument has found application in almost all branches of science and technology, in biological as well as metallurgical, solid state and polymer laboratories. A variety of models are now available, made by several small firms in addition to the main manufacturers of electron microscopes (the appendix in Hearle, Sparrow and Cross's book lists twelve makers).

For such a widely used instrument it is surprising that there has not been until now an authoritative handbook.

We have had survey articles and conference proceedings a-plenty, but no comprehensive treatment of the subject. At last the situation is on the way to being remedied. Oatley himself has written the first part of a monograph, dealing with the instrument itself, to be followed by an account of its applications and of some related instruments. With a simultaneity not unusual in scientific publication, a team of practitioners of the art now offer a more practical text on the operation and applications of the scanning microscope.

To some extent the two books cover the same ground: in respect of the design and operation of the instrument, and the interaction of electrons with a solid sample. Not unnaturally Oatley's treatment of these topics is much the more detailed. He gives, for example, thirty-four pages to the electron gun as against two in the other text, and twenty-six pages (two) on signal collection and amplification. The two treatments of electron interaction with solids have much more in common, especially on secondary emission, which is basic to so many applications. Surprisingly, Hearle *et al.* have little to say about back-scattering phenomena, but on the other hand they include the Auger electron and cathodoluminescence modes of image formation, not mentioned by Oatley. In all other respects the two books cover entirely different ground.

The chief emphasis in Oatley's monograph is on the physical principles and technical features of the instrument. The fundamental limitations on the design of a scanning microscope are thoroughly discussed in chapter 2, the component parts of it in the next chapter, and electron interactions in chapter 4. The final chapter considers the various mechanisms contributing to image contrast, and then the factors that determine image resolution when examining solid surfaces. The scanning transmission microscope is to be treated in part 2.

In the meantime Hearle, Sparrow and Cross have provided an excellent survey of the subject as a whole which, if short on its fundamental science and engineering, is long on its multifarious uses. After the opening chapters, there follow accounts of specimen preparation and instrument operation, and then of applications in metallurgy, fibres and polymers, solid-state electronics and biology. The remaining chapters deal with fault-finding, dimensional measurements (including stereography), management of a scanning microscopy unit (including how to cope with visitors) and the future of the subject. To cover such a wide field would be expecting too much of a single author. The three named on the book's title-page have found it desirable to call in other specialists, and in fact the team

of contributors numbers eight in all.

Their book is readable and well printed, illustrated with a wealth of micrographs, many of the high photographic quality for which scanning electron microscopy is renowned. There are bibliographies after each chapter and a useful list of manufacturers and suppliers at the end of the text. It will rank as the standard handbook for users of the instrument (the authors state there were more than 500 of them in 1970), at least until part 2 of the Cambridge monograph appears. Part 1 provides the requisite background of the physical principles, engineering design and parameters of performance of the microscope itself. Oatley's treatment is elegant, concise and up-to-date. It is not often that the progenitor of a new research technique of such importance spares the time and has the ability to write the definitive account of it.

V. E. COSSLETT

Weatherwise?

Weather Forecasting for Agriculture and Industry. Edited by J. A. Taylor. Pp. xix+250. (David and Charles: Newton Abbot, November 1972.) £5.75.

A RECENT symposium associated with the University College of Wales at Aberystwyth took as its subject the forecasting of weather for social and economic needs. The contributions have been collected into this attractive book of seventeen chapters, complete with diagrams, a selected classified bibliography and indexes.

The principal theme is weather forecasting from both the sending and the receiving ends, with the suggestion that improvements in the quality of the predictions must be matched with the more frequent and more intelligent and wider use of them. There are, this text makes clear, meteorological forecasts for all sorts of users but the main classes of beneficiary are farmers, industrialists, hydrologists and tourists. Apparently the meteorological services must aim at supplying more short- and longer-term predictions accurate enough, if fully and intelligently applied, to lessen the costs or raise the profits of economic concerns and reduce the risk of inconvenience to a comfort-demanding society.

Here the problem is discussed by twenty practitioners expert in at least one of the weather-sensitive activities. Not surprisingly, the cumulative result is attractive and authoritative. Longer-range forecasting will prove increasingly beneficial to research, management and decision-making in agriculture, to constructional engineering, and to the development of tourism. Other cognate themes incorporate weather influences in

the prediction of the demand for electricity, in the vagaries of river flow, and in the effect upon local climates of the extension of built-up areas. The many specific topics covered are exemplified by the discussions on the influence of weather upon the following: the daily attendance at recreational districts; the optimum amount of salt to prevent icing on roads; the growing of early potatoes; and fluctuations in the demand for beer.

Throughout the book the exposition and diagrams are lucid and effective, and the approach is highly practical and orientated toward cost-benefit and minimum-risk analysis. Most readers will quickly be convinced that in an intensifying economy with soaring prices of labour, goods and services, more attention should be given to applied meteorology in order to extract the maximum benefits and to ensure the minimum losses and waste in all weather-sensitive occupations. Undoubtedly the message set out here will attract and be easily intelligible to a wide range of persons, practical and academic. But it is to be hoped that an eleemosynary edition will be issued to bring the volume within the purchasing power of undergraduates who will also find the contents much to their liking. Then perhaps in their day they may convert the British from being a nation of indecisionists in weather affairs into a society that treats the financial and social aspects of climate with due respect and sanity.

ROBERT P. BECKINSALE

Molecular Structure

Molecular Geometry. By R. J. Gillespie. Pp. ix+228. (Van Nostrand Reinhold: London and New York, July 1972.) £4 cloth; £2 paperback.

The Electronic Structure of Organic Molecules. By T. E. Peacock. (*The International Encyclopaedia of Physical Chemistry and Chemical Physics.* Vol. 2. Topic 4. *Electronic Structure of Molecules.*) Pp. xi+145. (Pergamon: Oxford and New York, July 1972.) £3.50.

In the first of these two books Professor Gillespie has written the first full-length account of the valence-shell electron-pair repulsion theory of molecular geometry. The original introduction of the theory goes back to Sidgwick and Powell in 1940, but the greater part of its subsequent development is due to Nyholm and Gillespie himself. The basic idea is simple; it is that, except for very polar (near-ionic) systems, molecular shape is determined by the very large repulsions which exist between lone pair electrons and the

smaller repulsions between electrons involved in bonding in the valence shell of a central atom. No precise description of the detailed shape or character of these electronic orbitals is attempted: and indeed Gillespie sometimes speaks of an orbital as a part of a space or a region, and sometimes as an electron cloud.

After a few introductory chapters in which the basic ideas are expounded, most of this book is devoted to understanding particular geometries in the light of the fundamental rules. It is quite astonishing to see how successful the method is, even though adherents of the more traditional theories may be a bit horrified to be told that there may be up to eight distinct valence-shell orbitals. This part of the book is admirably illustrated, with a wealth of information not readily available elsewhere.

There are strengths and weaknesses in this theory. Its strength is that so much experimental information is so nicely rationalized. Its weakness is that it is not susceptible to any numerical application, and that it disregards so much that is well known about orbitals in atoms. Moreover, without considerable extension, it cannot tell us much about the shapes of molecules in excited electronic states. Its failure to take account of our knowledge of atomic orbitals explains why the theory cannot naturally explain such matters as the difference between the linear structure of HgCl_2 and the angular structure of BaCl_2 or the charge distribution in N_3^- , whereas simple considerations of hybridization do so. The book would have been much strengthened if a more effective link could have been provided between the new and the old theories.

Read in conjunction with traditional texts and particularly as regards the qualitative insights that it provides, this can be a very useful book.

Dr Peacock's book is of a different kind. His concern is with the traditional theory of π electrons; moreover the first two-thirds of the book is devoted to the Hückel theory, and only the last forty-four pages deal with improved theories. The material covered is similar to that in many previous texts; presumably this book was written because it was felt that the complete series needed something of this form. The volume has a rather old-fashioned character. The author is more at home with the calculation of energies than of other properties, and he provides some useful tables for comparison between theory and experiment. Unfortunately the proof-reading is atrocious, and there are rather more distinct errors than one would have wanted in a volume intended for student use.

C. A. COULSON

Pertinent Biology

The Future of Man. Edited by F. J. Ebling and G. W. Heath. (Proceedings of a Symposium held at the Royal Geographical Society, London, on April 1, 1971.) Pp. xviii+211. (Academic: New York and London, February 1972.) £2.80; \$8.95.

THE symposia of the Institute of Biology are now an established event in the biologist's calendar and one might therefore expect that they would have lost some of the originality that they undoubtedly had in the beginning. But if we look at the subjects of the Institute's symposia over the past decade we cannot help but recognize and applaud the freshness and pertinence of all of them to the contemporary scene. Some have seemingly been in response to current biological thinking, while others have anticipated a popular movement in the science. How apt, for instance, was the symposium on *The Natural History of Aggression* which had papers presented by people of Lorenzian calibre at a time when animal and human behaviour studies were at their height and were soon to be fascinating a wider public: then again, when the pesticide problem was coming into public prominence with all its attendant confusion, the Institute chose as its symposium's theme *The Problem of Birds and Pests*.

Through these ten years or so the Institute has managed to provide a timely focus for some of the chief biological problems of our time and because of this it has helped greatly to get matters into some sort of perspective. In my opinion the two most important of the symposia are the last two, *The Optimum Population for Britain* and the subject of the present volume *The Future of Man*. It is an example of the timeliness of these now famous symposia that this one in particular should have been held a year before the United Nations Conference on the Human Environment and further that the papers should be published in the same year of the Conference. One could almost say that, taken together, all the Institute's symposia would have made an excellent guide to those whose work it was to organize that United Nations Conference.

In *The Future of Man* the Institute of Biology has, as it were, arranged some peculiar bedfellows between its sheets, but they do represent a wide range of viewpoints of a problem. Of course all the papers are not of equal standard or even relevance, though this could be due more to the outlook of the reviewer than anything else. However, someone once said that all things are relevant and this is presumably the philosophy of the Institute. All in all, the papers published here are worth reading,

whether relevant to the general theme or not, and most are well written. One or two could have done with a little tighter editing and sometimes one gets the feeling that the selectors were trying to be a little too "with it" whereas the Institute of Biology has proved again and again that it can put together a fascinating assembly of people and views that are pertinent to our times.

ARTHUR BOURNE

Muscle

Muscle Biology: a Series of Advances. Edited by R. G. Cassens. Volume I. Pp. x+299. (Marcel Dekker: New York, August 1972.) \$17.50.

THIS, the first volume of a series, is based upon lectures delivered at the Institute for Muscle Biology, Madison, Wisconsin, in the spring of 1971. The respective chapters deal with the energetics of muscle contraction (Mommaerts), muscle regeneration (Carlson), enzyme kinetics (Taylor and Lymn), force-velocity relationships in heart muscle (Brady), muscle growth and atrophy (Goldberg), the effect of adrenergic neurotransmitters on the uterus (Marshall), spindle structure and function (Smith and Ovalle), muscle changes resulting from motoneurone dysfunction (Engel and Warmolts), the histopathology of human and chicken muscular dystrophy (Shafiq, Askanas, Asiedu and Milhorat) and oxygen uptake by striated muscle (Stainsby and Barclay). Each chapter gives a well-referenced review of current knowledge, and even though the book has been produced by a photo-offset method of reproducing typed manuscripts, the illustrations, which include a number of electron micrographs, are in general clear and of good quality. As is inevitable in any such multi-author work, the individual chapters vary considerably in scope but all contain material of considerable interest to biologists and to all who are interested in disease and dysfunction of cardiac and skeletal muscle. That by Smith and Ovalle, which occupies some eighty pages, is a particularly comprehensive survey of the structure and function of intrafusal muscle fibres. The appearance of subsequent volumes in this series will be awaited with interest; it is unfortunate that a book produced by a method designed to ensure rapid publication has taken so long to appear; one hopes that the editor and publisher will try to see that succeeding volumes are published more rapidly, as this is a field in which knowledge is advancing rapidly.

JOHN N. WALTON

Drug Problems

Dealing with Drug Abuse: a Report to the Ford Foundation. Pp. x+396. (Macmillan: London and Basingstoke, June 1972.) £3.95.

It would seem quite difficult, given what is known about drug-taking in America, to prepare a concise and reasonably objective description of the priorities for further research into treatment of drug users, public education in the field, and other relevant aspects of government policy. The *Summary of Findings, Conclusions, and Recommendations* (of the Drug Abuse Survey Project, financed by the Ford Foundation) provides just such a description. The present volume contains the Summary by Patricia M. Wald and Peter Barton Hutt) and seven staff reports.

The large existing literature notwithstanding, relatively little is known about how the various psychoactive drugs work nor about their long-term psychological and physiological effects. There is a particularly great need for interdisciplinary research (for example, relating the pharmacological and other effects to various social environments), study of the interaction of different drugs taken in combination, and experimentation with heroin use and maintenance.

The pharmacological literature on psychoactive drugs is summarized in a paper by James V. DeLong.

The second staff paper is a descriptive listing of drug education programmes, compiled by Patricia M. Wald and Annette Abrams. The main reported difficulty is a failure to evaluate suitably—or even to develop adequate criteria for evaluating—the very large number of efforts.

A paper by DeLong on treatment and rehabilitation is devoted largely to programmes for heroin users. Of various alternatives (for example, civil commitment, therapeutic communities, opiate antagonists), methadone maintenance programmes appear to be having the greatest overall "success".

"The Economics of Heroin" (by John F. Holahan) includes a description of international sources of supply and the likely effects in America of various alternative policy decisions.

A straightforward account of federal expenditures on drug-abuse control is provided in a staff paper by Peter Goldberg and DeLong.

Probably the most important and innovative paper is by Andrew T. Weil, "Altered States of Consciousness". The investigator begins with a clear summary of the relationship (or lack thereof) between the pharmacological and social/psychological effects of various classes of drugs. He then points out that drug experience can best be understood in terms of altered states of con-

sciousness, and he lists four proposals "to be kept in mind as the direction to move toward": recognition of the importance of altered states of consciousness and the existence of a normal drive to experience them; provision for the experience of altered states of consciousness in growing children; incorporation of the experience into society for positive ends; and encouragement of individuals to satisfy their needs for altered consciousness by means that do not require external tools.

The final staff paper, by Edgar May, provides a description and tentative evaluation of the British "system" of opiate-prescribing clinics. The treatment of addiction by medical rather than law enforcement procedures is speculatively regarded as having prevented a problem such as is seen in the United States.

HERBERT H. BLUMBERG

Origin of Spectral Lines

Radiation Transport in Spectral Lines. By G. Athay. Pp. xiii+263. (D. Reidel: Dordrecht, 1972.) 41 florins.

THIS book is one in a series of monographs which will each cover "a well defined and limited area of geophysics or astrophysics". Appropriately, this first volume is concerned with the fundamentally important area of energy transport processes and physical phenomena giving rise to spectral lines, and with methods used for extracting information concerning the medium in which the lines are formed.

In the introductory chapter the author suggests that spectral line studies in astrophysics may be divided into three phases. In the present phase, which resorts largely to numerical techniques for solving the radiative transfer equation, it is no longer necessary to suppress some of the physics of line formation in the interests of simplicity as is characterized by the earlier algebraic approach. Thus the detailed interplay of the different physical mechanisms involved in line formation is emerging and the information derived from spectral line studies is becoming more meaningful.

Much of the material presented is the result of computational work, a great deal of which has been done by the author and his colleagues. The book is to be regarded as a state-of-the-art progress report which, the author hopes, will be superseded by a treatise providing a more acceptable level in the ratio of physical insight to numerical results. The usefulness of the monograph is that it provides a valuable summary of some recent accomplishments in radiative transport problems and a guide to the directions for further research.

B. BATES

CORRESPONDENCE

Forestry Policy

SIR,—I would like to correct and put into proper perspective some of your statements about the Forestry Commission and its work (*Nature*, 241, 77; 1973). You assert, on the basis of the case presented in the consultative document *Forestry Policy*, that forestry is an "industry whose commercial value is zero or even less" and that "the scale of the Forestry Commission's research programme is an affront". Two main issues are raised—the commercial profitability of forestry in Britain and the nature and scale of research to be carried out by the commission.

First, with regard to what you describe as the commission's "pigheadedly bullish view of the scale of operations" it must be emphasized that the commission is not an independent body but simply an instrument of government policy, dependent on the three forestry ministers for instructions as to major objectives and the size of its planting programmes. Historical events have quite properly led the government to give the Forestry Commission different objectives at different times since it was established in 1919.

Forestry Policy does not make sufficiently clear the distinction between returns from investment in new planting compared with returns to be expected from exploitation of existing forest resources, including all the capital investment in the form of land, roads, drainage, fencing, and so on, which will persist for many years. As most of the Forestry Commission's new planting has taken place since 1948, the forests are still very young and as yet only a quarter of them are in production. Consequently, in order to estimate the rate of return on investment it is necessary to make assumptions about future levels of production, costs, and prices and to assign values to the social and environmental objectives which are difficult to express in money terms. We can forecast future production with some confidence, but clearly future costs and prices are less predictable. The cost/benefit study assumed constant 1972 costs and prices, although some authorities believe that the real price of wood will continue to rise following the historical trend. No attempt was made to quantify the human, the social and the environmental benefits from forestry apart from the provision of employment and benefits to recreation.

On the assumptions mentioned above, new planting in Britain can

earn up to 3 per cent, a rate which is generally higher than that obtained in other countries in temperate regions of the Northern Hemisphere. When one considers the continuing management of the existing forest estate, the financial returns are estimated to be much higher, probably of the order of 9 per cent. It is estimated that at current prices the revenue from the timber which will eventually be harvested from forests, which as you state have cost £405 million, will be about £1,000 million. (The eventual value of the timber should not be confused with its discounted value.) Although the discounted value of the harvested timber will be less than the cost of £405 million, this is a reflexion of the fact that forest investment from the time of inception in 1919 has been charged a rate of interest on the capital advanced which is greater than the actual rate of return to be expected from new forests in the northern temperate zone, although this return will eventually be positive.

The editorial argues that because of the commercial unprofitability of forestry in Britain, the scale of the commission's research programme is unjustified. However, regardless of the profitability or otherwise of forestry, so long as it continues on an extensive scale there will be a need for research and development to achieve increased efficiency. I should point out that only some 75 per cent of the research division's expenditure relates directly to research and development—the balance being devoted to service-type work including publications, statistical and photographic services, seed supply and advisory and consultative work for management.

There are great opportunities for improvement of timber production and cost-effectiveness by planned research, and it is noteworthy that much of the present effort is directed to existing forests and their management. Also, much of the research is now proving to be of great value in relation to planning and conservation interests, landscaping and recreation management, and to planting and management of trees in the countryside as a whole.

The editorial article asserts that the research programme of the Forestry Commission remains "scandalously immune" from the application of the Rothschild principles. In fact, for many years a major feature of the commission's research and development programme has been the close liaison between research personnel and forest managers in both the national

and private sectors—a relationship that is envied by the forest departments of many other countries. In very few countries of the world is current practice so closely related to research findings as is the case in Britain. With few exceptions, the research is directed to practical ends and often consists of technical development aimed at solving immediate problems. Thus, I believe that the relevance of the commission's research and development, and its impact on practice, are attributable to a linkage with management which accords well with the Rothschild principles.

Your comment that the work on genetics, physiology and pathology has little relevance to the immediate future is ill-informed. It should be apparent from the annual report on research that the pathology programme is concerned with immediate and pressing problems including, for example, Dutch elm disease and butt rot; the physiology programme is much concerned with speedy propagation of improved clones and problems of root growth relating to the vigour and wind stability of trees; and its genetics programme is largely a practical process of selection and breeding for better form and vigour. The statement that much of the Forestry Commission's research programme is concerned with the collection and testing of seed and the improvement of germination is incorrect since in fact it accounts for less than 1 per cent of its research expenditure.

Looked at in retrospect, the commission's research work has an outstandingly good record in terms of its relevance to practice, and a considerable part of the credit for the achievements of British foresters in enabling degraded soils to support a production crop, in the successful choice of species, and in improved protection and production techniques must go to the research on which they are based.

Possibly it is the immediate practical relevance of the commission's research which led you to refer to parts of the programme as "hum-drum". If so, the commission need make no apology. Ironically, the commission's research programme has, in fact, sometimes been criticized for giving too much weight to the solution of immediate practical problems and paying insufficient attention to long term basic research, which led the Trend Committee to recommend that more basic forestry research should be carried out by a research council. This recommendation has been put into effect with the establish-

ment of the Natural Environment Research Council's Institute of Tree Biology. It would seem singularly inappropriate, therefore, to suggest now that the "more interesting" parts of the Commission's own research programme might be transferred to the research councils. Moreover, such transfer could hardly be justified on the grounds that the research councils are commercially profitable.

Yours faithfully,

PHILIP WAREING

*Department of Botany and
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Aberystwyth*

These arguments are not compelling.

(1) Professor Wareing's suggestion that existing forests would be commercially profitable if the capital cost of their formation had been saddled with an interest rate equal to the expected rate of return from new planting does not invalidate the assertion that this rate of return is much less than that expected from other forms of public investment, by which test the Forestry Commission's operations are unequivocally unprofitable.

(2) On the scale of the Forestry Commission's research programme, the question raised in *Nature* was whether a research programme costing 8 per cent of the commission's annual operating budget is justifiable when there is no prospect that research and development will make the publicly owned forests commercially viable in the ordinary sense.

(3) It is true and was acknowledged that much of the research is relevant to other aspects of British life, but the question is whether, in those circumstances, the appropriate customer (in Rothschild's sense) should be some

other organization than the Forestry Commission.

(4) Humdrum was not intended to apply to the practical work of the research division of the Forestry Commission but to such projects as the attempt to quantify the recreational benefits of British forests, referred to in the leading article of which Professor Wareing complains.

Editor, *Nature*

Addendum

A NOTE in proof should have been added to the article "Action of Black Widow Spider Venom on an Aminergic Synapse" by S. G. Cull-Candy, H. Neal and P. N. R. Usherwood (*Nature*, 241, 353; 1973) as follows: "N. Kawai, A. Mauro and H. Grundfest (*J. Gen. Physiol.*, 6, 650; 1972) have recently shown that BWSV also acts on lobster nerve-muscle synapses, thus confirming our report of the action of this toxin on non-cholinergic systems".

Erratum

In the article, "Further Investigations of the Transfer of Bomb ^{14}C to Man", by D. D. Harkness and A. Walton (*Nature*, 240, 302; 1972), the following corrections should have been made. Paragraph 1, line 7, should read "... differed significantly from ..."; paragraph 5, the equation should read "... $D_t = 0.95 A_t + 0.05 O_t$ "; paragraph 5, lines 8-10, should read "It seems that the finite turnover time for carbon in the human body can be measured via the temporal changes in environmental ^{14}C concentrations"; paragraph 7, penultimate sentence, should read "Smaller variations in the ^{14}C concentrations in individual soft tissues, although statistically not significant, do appear to be real".

Reports and Publications

not included in the Monthly Books Supplement

Great Britain and Ireland

Local Government Boundary Commission for England—Report No. 1. (Cmnd. 5148.) Pp. vii+217+10 maps. (London: HMSO, 1972.) £1.30 net. [2211]

Report of the Agricultural Research Council for the year 1971-72. Pp. vi+68. (London: HMSO, 1972.) 47p net. [2411]

Medical Care in Developing Countries. Pp. 40. (London: Office of Health Economics, 1972.) 25p. [2411]

London and Home Counties Regional Advisory Council for Technological Education. Science Education in the Region, 1972/73. Pp. 52. (London: London and Home Counties Regional Advisory Council for Technological Education, 1972.) 50p. [2411]

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